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ART. XVIII.—*Michael Faraday, his Life and Works;* by Professor A. DE LA RIVE.*

SCIENCE has just lost one of its most eminent and faithful representatives. Faraday died on Sunday, the 25th of August, 1867, at Hampton Court; he was born on the 24th of September, 1791, at Newington Butts, near London. In 1804, at the age of thirteen, he was apprenticed to a book binder, in whose workshop he remained eight years. So many books passed through his hands that he could not resist the temptation of opening and reading some of them. These readings, performed in the evenings after the work of the day was finished, gave him a taste for study, and in particular for that of the sciences. The *Encyclopædia Britannica* first of all introduced him to some notions of electricity; and it was afterward, from the works of Mrs. Marcet, that he derived his first knowledge of chemistry. His labors received their permanent direction from this opening; their essential objects were electricity and chemistry.

"Do not fancy," he said to me in a letter† of the 2nd of October, 1858, in which he gives me these details, "that I was a profound thinker or a precocious child; I had merely a good deal

* Copied from the Phil. Mag. for Dec. 1867, for which it was translated from the Bibliothèque Universelle, October 25, 1867, Arch. des Sci., pp. 131–176.

† This letter was addressed to me on the occasion of the death of Mrs. Marcet, and the notice which I was about to publish on this distinguished woman (see Bibl. Univ. nouvelle série, 1858, vol. iii.).

of life and imagination, and the tales of the Thousand and One Nights pleased me as much as the *Encyclopædia Britannica.*

"But what saved me was the importance I early attached to facts. In reading Mrs. Marcet's book on chemistry, I took care to prove every assertion by the little experiments which I made as far as my means permitted ; and the enjoyment which I found in thus verifying the exactitude of the facts contributed essentially to give me a taste for chemical knowledge. You may therefore easily imagine the pleasure I experienced when I subsequently made the personal acquaintance of Mrs. Marcet, and how delighted I was when my thoughts went backward to contemplate in her at once the past and the present. Whenever I presented her with a copy of my memoirs, I took care to add that I sent them to her as a testimony of my gratitude to my first instructress."

"I have the same sentiments toward the memory of your own father," adds Faraday ; "for he was, I may say, the first who encouraged and sustained me, first at Geneva when I had the pleasure of seeing him there, and afterward by the correspondence which I regularly maintained with him."

Faraday here alludes to a journey in which he accompanied Davy to Geneva in 1814, and in which, during a stay which he made with his illustrious master at my father's, the latter quickly discerned the merits of the young assistant, and formed relations with him which were interrupted only by death. At the time when he travelled with Davy, Faraday was his assistant at the Royal Institution in London ; and I must say that he has more than once expressed to me, both by letter and *viva voce*, his thankfulness to the eminent chemist, who had admitted him to one of his courses, and consented, after running through the notes of this course prepared by the young pupil, to take him for his assistant.

After the journey just referred to, Faraday, with the exception of rare and short absences, never again quitted the Royal Institution, where he had his laboratory and his residence. Married to a lady worthy of him, and who shared and understood all his impressions and all his sentiments, he passed a life equally peaceful and modest. He refused all the honorary distinctions which the government of his country wished to confer upon him ; he contented himself with a moderate salary and with a pension of £300 sterling, which fully sufficed for his wants ; and accepted nothing supplementary to this except the enjoyment, during the summer, in the latter years of his life, of a country house at Hampton Court, which the Queen of England graciously placed at his disposal.

Without children, a complete stranger to politics or to any

kind of administration, except that of the Royal Institution, which he directed as he would have directed his own house, having no interest but that of science, and no ambition but that of advancing it, Faraday was of all *savants* the one most completely and exclusively devoted to the investigation of scientific truth of which the present century offers us an example.

One may easily understand what must be produced under such circumstances by a life thus wholly consecrated to science, when to a strong and rigorous intellect is joined a most brilliant imagination. Every morning Faraday went into his laboratory as the man of business goes to his office, and then tried by experiment the truth of the ideas which he had conceived overnight, as ready to give them up if experiment said *no*, as to follow out the consequences with rigorous logic if experiment answered *yes*. His every-day labor experienced no interruption, except the few hours which he devoted from time to time to the exposition in the theatre of the Royal Institution, before an audience equally numerous and select, of certain parts of physics and chemistry. Nothing can give a notion of the charm which he imparted to these improvised lectures, in which he knew how to combine animated and often eloquent language with a judgment and art in his experiments which added to the clearness and elegance of his exposition. He exerted an actual fascination upon his auditors; and when, after having initiated them into the mysteries of science, he terminated his lecture, as he was in the habit of doing, by rising into regions far above matter, space, and time, the emotion which he experienced did not fail to communicate itself to those who listened to him, and their enthusiasm had no longer any bounds.

Faraday was, in fact, thoroughly religious, and it would be a very imperfect sketch of his life which did not insist upon this peculiar feature which characterized him. His Christian convictions occupied a great place in the whole of his being; and he showed their power and sincerity by the conformity of his life to his principles. It was not in arguments derived from science that he sought the evidences of his faith; he found them in the revealed truths at which he saw that the human mind could not arrive by itself alone, even though they are in such great harmony with that which is taught by the study of nature and the marvels of creation. Faraday had long and justly perceived that scientific data, so movable and variable, cannot suffice to give to man a solid and impregnable basis for his religious convictions; but he at the same time showed by his example that the best answer which the man of science can give to those who assert that the progress of science is incom-

patible with these convictions, is to say to them, "And yet I am a Christian."

The sincerity of his Christianity appeared in his actions as much as in his words. The simplicity of his life, the rectitude of his character, the active benevolence which he displayed in his relations with others, gained him general esteem and affection. Always ready to render services, he could quit his laboratory when his presence elsewhere was necessary to a friend or useful to humanity. We see him putting his knowledge under contribution both for inquiries upon questions of public health or industrial applications, and to give practical advice to an artisan or examine the discovery of a *débutant* in the scientific career. Only, as I have already said, with these exceptions, he made it a rule not to allow himself to be turned aside from the labors to which he had consecrated his life by occupations of another kind, or by those pretended duties of society which waste time, abridge intellectual life (already so short), and very often leave nothing behind them but emptiness and regret. It was not that he could not be eminently sociable when necessary, or that he did not allow himself some relaxations when, fatigued with work, he needed some repose. But these were only accidental circumstances in his life, which was so exclusively devoted to his laboratory.

The scientific career of Faraday was equally fortunate and complete. Named as early as 1823 a correspondent of the Academy of Sciences of Paris, he was called in 1844 by this same Academy to occupy one of its eight foreign Associateships, after having been associated successively with all the learned bodies of Europe and America. He was by no means insensible to these scientific honors, which he accepted with genuine satisfaction, whilst he constantly refused every other kind of honorary distinction.

But it is time to commence the more important part of this notice, that which is to be devoted to the examination of the works of Faraday. Only I may perhaps be allowed, before speaking of the works themselves, to say a few words of the manner in which Faraday worked.

Is it true that the man of science who wishes to interrogate nature must set himself face to face with his apparatus, make them act to derive facts from them, and wait until these facts have appeared, in order to deduce their consequences, and all without any preconceived idea? Most certainly the philosopher who could advance such an opinion has never experimented, and in any case this method has never been that of discoverers; it was assuredly not the one adopted by Faraday.

There is a second method also which was not his, although

it is truly worthy of attention, and often fertile of results. This consists in taking up known phenomena and studying them with great precision, carefully determining all the elements and numerical data, so as to deduce therefrom the laws which govern them, and often also to show the inexactitude of the laws to which they were supposed to be subjected. This method requires great previous study, great practical talent in the construction of apparatus, remarkable sagacity in the interpretation of the results furnished by experiment, and, lastly, much perseverance and patience. It is true that it leads with certainty to a result; and this is its good side; but the difficult conditions which it imposes are so many obstacles which prevent its being generally followed, except by the highest intellects.

A third method, very different from the last mentioned, is that which, quitting the beaten track, leads, as if by inspiration, to those great discoveries which open new horizons to science. This method, in order to be fertile, requires one condition—a condition, it is true, which is but rarely met with,—namely genius. Now this condition existed in Faraday. Endowed, as he himself perceived, with much imagination, he dared to advance where many others would have recoiled; his sagacity, joined to an exquisite scientific tact, by furnishing him with a presentiment of the possible, prevented him from wandering into the fantastic. Still always wishing for facts, and accepting theories with difficulty, he was nevertheless more or less directed by preconceived ideas, which, whether true or false, led him into new roads, where most frequently he found what he sought, sometimes indeed what he did not seek, but where he constantly met with some important discovery.

Such a method, if indeed it can be called one, although barren and even dangerous with mediocre minds, produced great things in Faraday's hands; thanks, as we have said, to his genius, but thanks also to that love of truth which characterized him, and which preserved him from the temptation so often experienced by every discoverer, of seeing what he wishes to see, and not seeing what he dreads.

The works which have issued from his brain, so well organized, are numerous and varied; they relate essentially, as we have already stated, to chemistry and electricity. Those on the latter subject are by far the most numerous and important; we shall therefore devote to them the greater part of this notice, after giving a summary exposition of the others.

I.

In 1816, Davy received a specimen of native caustic lime from Tuscany. He gave it to Faraday for analysis, and found that the account given was so perfect that he had it printed, and accompanied it with some observations. This success, by giving Faraday confidence in his own strength, encouraged him to attempt other original researches. He published (in 1817 and 1818) an investigation of the passage of gases through narrow tubes, from which it appeared that the velocity of the flow of elastic fluids does not depend upon their density alone, but also upon their individual nature. Various other points of chemistry and physics, besides those which had electricity and magnetism for their object, attracted his attention from time to time throughout the whole of his scientific career. Now we have a note upon the combustion of the diamond; then an investigation of the sounds produced by the combustion of gases, or by the superposition of a strongly heated iron rod upon a mass of copper at the ordinary temperature (Trevelyan's experiment); and then, again, researches upon the limit of vaporization, or upon the evaporation of mercury at low temperatures. We may notice two important memoirs—one upon the explanation of certain optical illusions produced by bodies in motion, the other describing some new acoustic figures proceeding from the vibrations of the stratum of air in contact with the surface of vibrating plates. His elegant discovery of *regelation* (that is to say, of the power possessed by two fragments of ice when brought together to become amalgamated by the fact of their simple contact at a temperature above 32° F.), followed into its consequences as it has been by Tyndall, has had a much greater influence than perhaps he ever expected. In all these notices, even the least important of them, we find an original idea, a new and striking point of view, which enables us at once to recognize Faraday. And, in connection with this, how can we omit to mention his simple and clear explanation of table-turning, and the ingenious experiment by which he so clearly shows the muscular efforts made unconsciously by the persons who, by laying their hands upon the table, cause its movement?

Let us now dwell for a few moments upon some researches of longer duration, the publication of which preceded, and also in great part accompanied, his great works on electricity.

In 1820 Faraday described two new compounds of chlorine and carbon. One of them is solid, transparent, and colorless; it crystallizes in little prisms and in laminæ, and is obtained by exposing to the direct action of the sun bicarbonated hydrogen gas with a large proportion of chlorine. The other con-

tains less chlorine ; it is liquid and colorless, possesses great density, and is prepared by passing the former through an incandescent tube, from which chlorine is set free. The discovery of these two compounds filled up an important gap in the history of chemistry.

Subsequently (in 1825), by the compression of the gas obtained from coal, Faraday obtained a new compound, which, no less interesting than the preceding from a scientific point of view, had besides a great industrial importance. This was a bicarburet of hydrogen in a liquid state, which was found to be a mixture of several compounds endowed with various degrees of volatility, and which could be separated by distillation. Every one knows the advantage, in the production of colors, derived from this by the illustrious chemist Hofmann, when he extracted aniline from it.

The discovery of this bicarburet of hydrogen was only an incident in the researches which Faraday had undertaken in 1823, upon the condensation of gases into liquids. His mode of operation in this investigation consisted in placing, in one extremity of a recurved tube closed at both ends, the necessary ingredients for the production of the gas, and plunging the other extremity in a freezing mixture. The gas, evolved in a closed space, speedily condensed into a liquid state in the refrigerated extremity of the tube. In this way chlorine, sulphurous acid, sulphuretted hydrogen, carbonic acid, protoxyd of nitrogen, cyanogen, ammonia, and hydrochloric acid were successively reduced to a liquid state. With the exception of chlorine, all these liquefied gases were colorless and perfectly transparent; and all of them had a refractive power superior to that of water. The attempts made to reduce the other gases, especially hydrogen, oxygen, and nitrogen, to a liquid state were fruitless. Twenty years later (in 1844) Faraday resumed these experiments by directly condensing the gases by mechanical processes in very strong and hermetically sealed tubes, refrigerating them by means of the mixture of ether with solid carbonic acid produced by Thilorier's method. The condensation could be brought to fifty atmospheres, and the lowering of temperature to $-166°$ F., or 110° C. below 0°. In this way Faraday succeeded in liquefying, besides the gases which I have already mentioned, olefiant gas, phosphuretted hydrogen, and arseniuretted hydrogen, as also fluosilicic acid; but he did not succeed in solidifying them. On the other hand, by applying his new process to the gases which he had previously liquefied, he brought them not only to a liquid state, but even to that of transparent and crystalline solids; hydrochloric gas alone of these latter would not become solid, whilst hydriodic and hydrobromic gas were successively liquefied and solidified.

It is easy to understand all the importance of an investigation the result of which was to modify completely, the received ideas as to the constitution of the permanent gases by causing them to enter into the category of simple vapors; this was to introduce into molecular physics a new and important notion, the consequences of which have gradually unfolded themselves.

It is also to a question of molecular physics that we must refer the memoir on the relations of gold and the other metals to light, published by Faraday in 1857. Among other interesting facts that this memoir contains, we shall cite that of a leaf of beaten gold, which, when placed upon a plate of glass, becomes perfectly transparent and colorless when it is brought to a high temperature, and which, when seen by transmitted light, resumes its green color when subjected to strong pressure. A great number of experiments upon the pulverulent deposits of various metals obtained by electrical discharges transmitted through very fine wires, led to remarkable results as to the variations of color arising from change in the molecular state of the same body. We also find in this memoir a detailed investigation of the various colors presented by different solutions of gold, and especially of the fine ruby-red tinge obtained by the solution of a quantity of gold which, if agglomerated into a single mass, would not occupy the seven-hundred-thousandth part of the volume of water which it colors. It is not necessary to dwell upon the interest presented by researches having for their object the study of the influence, still so imperfectly known, of the molecular structure of bodies upon their relations to light, and especially upon their transparency.

Among the numerous works of Faraday relating to the applications of science to the arts, we shall confine ourselves to citing his researches upon the manufacture of steel, and of glass for optical purposes, these being the most important.

It was by the analysis of the Indian steel called *wootz* that he was led, in concert with Stodart, to compose an alloy which had all the properties of this, by combining aluminium with iron and carbon. In a letter addressed in 1820 to Professor De la Rive,* he relates all the attempts made by his collaborateur and himself during two years of persevering labor, to discover the most satisfactory alloys. He indicates, as one of the best, that of rhodium and steel, and, as presenting curious peculiarities, that of steel and silver; this last alloy does not become a true combination unless the silver only forms $\frac{1}{500}$ part of it. Platinum, on the contrary, combines in all proportions with steel, but it does not furnish so good an alloy as rhodium and silver for the construction of cutting-instruments.

* See Bibl. Univ. (1820), vol. xiv, p. 209.

Although interesting in many respects, the results which Faraday obtained in his great investigation of the alloys of steel were not proportionate in their importance to the time and trouble which they cost him. We may say the same of the laborious researches upon the manufacture of glass for optical purposes, which he made a few years afterward (in 1829). It was upon the initiative taken in 1824 by the Royal Society of London, which named a committee for the study of the improvement of glass, with a view to its optical use, that Faraday was called upon to occupy himself with it. Whilst he pursued the chemical part of these investigations, Dollond worked up the glass, and Herschel subjected it to the test of experiment. At the end of long and difficult experiments, Faraday ascertained that the greatest difficulty in the way of the fabrication of a good flint glass (that is to say, a very refractive glass) was the presence of streaks and striæ proceeding from a want of homogenity, due, in its turn, to differences of composition between the contiguous portions of the same glass. The employment of oxyd of lead in the composition of flint glass was the cause of this defectiveness, which could not be avoided even by making use of the most efficacious means of rendering the mixture perfect while in a state of fusion. Among the combinations tried, that of borate of lead and silica furnished a glass endowed with optical properties still more strongly marked than those of flint glass, and at the same time presenting a very uniform structure. This glass, which, on account of its great density (double that of flint glass), has been named heavy glass, is found unfortunately to have a slight yellowish coloration which renders it unfit for optical purposes; but the labor which Faraday devoted to its fabrication has not been lost; for, as we shall see hereafter, this same glass, in the hands of the talented experimenter, became the instrument of one of his most beautiful discoveries.

In the long and curious memoir which he published upon the fabrication of optical glass, Faraday gives a minute description of all the processes employed by him, of the construction of furnaces, selection of crucibles, means of heating, various artifices, such as the injection of platinum in powder into the fused glass to cause the disappearance of bubbles, &c. It is a genuine instruction in chemical manipulation, and, as it were, a complement to his Treatise on this subject, which was published in 1827, and has since gone through three editions. Only those who are called upon to experiment in the domains of physics and chemistry can appreciate the immense service which this treatise has rendered to them, by teaching them a multitude of processes of detail so valuable for them to know,

and of which a description was previously nowhere to be found, so that every one was obliged to undergo an apprenticeship to them on his own account. It was necessary that a *savant* who for so many years had been struggling with the difficulties of experimentation, and who had been able to surmount them in so ingenious a manner, should give himself the trouble to describe the means which he had employed, so that his experience might be of service to others. Faraday was this *savant*, and his object was completely attained.

Here, perhaps, before proceeding to another set of subjects, we ought to speak of certain of Faraday's theoretical ideas relating to general physics, and more especially to the nature of the forces, and their correlation to each other and to the essence of matter; but we prefer not to discuss the opinions emitted by him upon these questions until after the exposition of his works on electricity and magnetism. We must, however, at once admit that his views on these matters are very contestable, and that, if they inspired him to make experimental researches of the highest interest, this is a proof that, in the hands of a man of genius even a bad theory may be the origin of the most beautiful discoveries.

II.

I pass now to the examination of those works of Faraday which relate to electricity and magnetism. It is not without embarrassment that I approach this examination; for these researches are so numerous that it would be necessary to extend this notice beyond all bounds in order to give only a simple analysis of them; and they are at the same time so varied, that it is impossible to explain them in the chronological order of their publication without confusion being the result. Thus, for example, the researches on induction are interrupted by others on electrochemical decomposition, to be afterward resumed and completed. Each memoir certainly forms a complete whole; but one memoir is most frequently followed by another, the subject of which is quite different. It seems as if the author, after having treated one question, found it necessary to recollect himself before resuming it, and to divert his mind from it, so to speak, by taking up some other kind of work.

It has therefore appeared to me that the best thing for me to do, was to group all these various works under a few distinct heads, so as to be able to give their essence without requiring to enter into too many details. The first would include all the researches relating to electrochemistry; the second, those which have for their object induction, whether electrodynamic or electrostatic; and the third, the phenomena relating to the

action of magnetism and dynamic electricity upon light and upon natural bodies in general. It is true that there are some works which elude this classification, as they will not enter into any one of our three divisions. But these are less important works, and such as were produced as occasions offered; that is to say, they are the fruit of some particular circumstances which attracted Faraday's attention to some special point. Such is, for example, the memoir which has for its object, the investigation of the electrical properties of the *Gymnotus*,—and that devoted to the evolution of electricity by the friction exerted against solid bodies by the globules of water or other substances carried up by vapor—experiments undertaken in consequence of the invention of Armstrong's machine. Lastly, there are others which only contain the more or less indirect consequences of the fundamental discoveries, which will be explained in one of the three subdivisions under which we have grouped them. We shall not dwell upon any of these, thinking that we may give a more exact and complete idea of all the progress which Faraday caused the science of electricity and magnetism to make by confining ourselves to pointing out in some detail the most prominent parts of his researches upon these subjects.

Faraday commenced with chemistry in his scientific career; it is therefore not surprising that he approached electricity by the study of electrochemistry. It was, moreover, toward electrochemistry that his attention must have been first directed in that laboratory of the Royal Institution which had witnessed the magnificent discoveries of Davy in chemical decompositions effected by the pile, and especially in the production of the alkaline metals. In taking up this subject, Faraday only followed the traditions left to him by his predecessor.

His researches upon the electrical conductibility of bodies constitute a first step in this path. The business was to ascertain whether, as was previously supposed, the presence of water is necessary to render solid bodies conductors, and whether solid non-metallic (and consequently compound) bodies can conduct electricity without being decomposed. Commencing with water, which is an insulator when solid and a good conductor in the liquid state, Faraday shows that a great number of compound substances are in the same case. Such are the many oxyds, some chlorids and iodids, and a multitude of salts, which do not conduct electricity in the solid state, but, without any intermixture of water, become excellent conductors when liquefied by heat, and are not decomposed by electricity with separation of their elements in the same way as aqueous solutions. To the list of these compounds Faraday adds that of

those substances, either simple, like sulphur and phosphorus, or compound, such as the periodids and perchlorids of tin, and many others, which continue isolators when fused as well as in the solid state. In this first investigation, notwithstanding a great number of experiments in which he employed the influence of heat and of electricity of high tension in the study of the conductive power of solid bodies, he did not succeed in determining very accurately the conditions of electrical conductibility; he only ascertained that, with one exception, which he justly regards as only apparent, there is not a solid body which, on becoming conductive by its passage to a liquid state, is not decomposed by the electrical current. We may add, so as not to return to the subject, that Faraday sometimes had doubts upon this point, and he even thought that water could conduct electricity without being decomposed. Now experiment shows that in all cases, even those which appear most favorable to this opinion, electricity cannot be transmitted under any form through a compound liquid body, without this body undergoing electrochemical decomposition.

As to the causes of conductibility, they are still far from being known; when we see bodies, such as the gases, becoming conductors when greatly rarefied, whilst under the ordinary pressure they are perfect insulators, we are compelled to come to the conclusion that the impossibility that we find of explaining this difference, as well as so many others presented in this respect by solid and liquid bodies, is due to the fact that we have not yet a correct notion of the molecular constitution of bodies. Perhaps the recent theories of several physicists, particularly that of Clausius, who regards the particles of bodies as being in a constant state of movement, may succeed in elucidating this subject, which is still so mysterious. Faraday himself had fully foreseen this relation between electrical conductibility and the ideas which we may form as to the nature of matter. In a remarkable article published in 1844 he showed, upon an experimental basis, that, in the theory according to which a body is regarded as consisting of atoms possessing weight separated from each other by larger or smaller intermolecular intervals, there are a multitude of facts, some of which can only be explained by assuming that the atoms are the conductors and the molecular space an insulator, and the others by supposing that the intermolecular space is the conductor, and the atoms insulators—a contradiction which is inadmissible. He concluded from this that we must imagine matter to be continuous, or rather imagine the atoms to be simply centers of force, and consequently replace the atomistic by the dynamical theory. We shall often find traces of these

ideas in the subsequent works of Faraday; for ourselves we cannot take this view. We are convinced that it is not by denying the existence of matter, properly so called, and admitting only that of forces, that we shall succeed in solving the difficulties under consideration and many others, but rather, following the example of Clausius and others, by modifying the ideas hitherto accepted as to the mode of constitution of bodies and replacing them by others more in accordance with recent discoveries.

But we must return to electrochemistry. I have already said that Faraday first occupied himself with chemical decompositions effected by the electrical current. He commences by effecting the decomposition of water and of solutions by means of a jet of ordinary electricity, rendered as continuous as possible by leaving a stratum of air interposed between the metallic points which convey and carry off the electricity from a machine, and a strip of moistened paper which this electricity traverses; he observes that the deposition of the elements separated from the decomposed liquid takes place against the surface of the air which is in contact with the paper. Then, investigating the decompositions effected by the pile, he examines the various explanations which have been given of this phenomenon, and concludes that it is much rather a chemical phenomenon than a truly electrical one. In other words, it is a peculiar form of affinity which, under the influence of electricity is exerted between the neighboring molecules, so that the decomposition is the easier in proportion as the affinity is stronger. He shows that the transfer of the elements can only take place between bodies, the constituent parts of which have an affinity for each other; and if these elements separate in a free state against the surface of the metallic poles of the pile, this is because they cannot combine with the substance of these poles; for whenever this combination is possible, they are no longer set free. Water in some cases, air in others, as we have already seen, may serve as poles just as well as solid bodies. Faraday justly rejects the old idea of certain physicists who attributed electrochemical decompositions to the ordinary electrical attractions and repulsions exerted upon the elements of a conductive liquid by the voltaic poles immersed in it. The metallic wires, or other conductors, which transmit electricity into a liquid, are merely, according to him, the roads by which the electric current passes into the liquid; therefore, to exclude any idea of electrical tension which is more or less implied in the name *pole*, Faraday proposed to substitute for the denomination *poles* that of *electrodes*. He likewise applied the term *electrolysis* to the chemical decomposition effected by electricity, reserving that

of *analysis* for the ordinary chemical decompositions in which electricity does not assist. Lastly, he gives the name of *electrolytes* to those compound bodies which are capable of being decomposed by the electric current.

After this preliminary and general study of the subject, Faraday enumerates the results which he obtained by submitting to electrochemical decomposition a very great number of compounds, some of them simple acids or simple bases, others saline combinations. He dwells particularly on the secondary effects often manifested in these decompositions, especially in the case of aqueous solutions, in which decomposition of the water and of the substance dissolved takes place at the same time. But the essential point of his researches is the law at which he arrived as to the definite nature of electrochemical decomposition. He demonstrates, relying solely upon experiment, that the quantity of chemical action exerted by an electrical current is proportionate to the quantity of electricity constituting this current, and, further, that the same quantity of electricity, or the same current, decomposes chemically equivalent quantities of all the compound bodies through which it is passed. Thus, if we place one after the other, in the circuit of a voltaic pile, several pieces of apparatus arranged for the decomposition of water and for collecting the gaseous products of this decomposition, we find that in all, even when the degree of acidity of the water and the form and size of the electrodes are different in each, the same current traversing them for a given time produces the same quantity of gas, and consequently decomposes the same quantity of water. The quantity of water decomposed in a given time, appreciated by the quantity of gas evolved, is therefore the exact measure of the quantity of electricity which has produced this effect. Hence, like Faraday, we give the name of *voltameter* to the very simple apparatus which holds acidulated water destined to be decomposed by the current, and by means of which the volume of gases set free by this current in a given time may be exactly measured.

The second principle, that the same quantity of electricity decomposes chemically equivalent quantities of all compound bodies, was demonstrated by Faraday by placing several different electrolytes one after the other in the same circuit, as, for example, acidulated water in a voltameter, and protochlorid of tin and chlorid of lead in a state of fusion; and he obtains quantities of tin, lead, chlorine, hydrogen and oxygen which are chemically equivalent. Then, rising from the effect to the cause, he comes to the conclusion that there is a perfect equality between the electricity which decomposes a body and that which is generated by the chemical action which produces the direct

decomposition of an equal quantity of the same, or of a chemically equivalent quantity of some other body. He is thus led to pay attention to the theory of the pile, and to recognize that the power of this apparatus originates in chemical action, and not in the contact of two heterogeneous metals—a contact which is not necessary either to produce a spark or to cause a chemical decomposition.

He establishes in the first place, that, either to effect a decomposition or to produce a spark, a plate of zinc immersed in acidulated water is sufficient without its being necessary to bring the zinc into contact with any other metal. He shows that in every pile the presence of an electrolyte (that is to say, a liquid susceptible of being decomposed) is indispensable for the evolution of electricity. Then, distinguishing in the electricity generated the intensity (or the tension) and the quantity, he studies the circumstances, depending either on the nature of the chemical action or the number of voltaic pairs associated, which exert an influence on these two characters of the current. In a word, he establishes such a correlation between that which occurs in the interior of a pile and that which takes place in the electrolyte interposed between the poles of this pile, that it is impossible not to admit (with him) that electrolytic decomposition is nothing but a form of chemical affinity transferred from the pile into the electrolyte decomposed.

Wishing to obtain an idea of the quantity of electricity which is associated with the particles of which matter is composed, he endeavors to estimate that which is necessary for the decomposition of a grain of water, regarding it, as he is justified in doing, as equivalent to that produced by the direct chemical action (of the acidulated water upon the zinc) which decomposes this grain of water. He arrives at this incredible result—namely, that this quantity of electricity, appreciated by the heat evolved by it in traversing a fine platinum wire, is superior to that manifested in 800,000 discharges of a battery of Leyden jars, charged by thirty turns of a powerful plate-machine, and consequently equivalent to that constituting a violent flash of lightning.

The researches of which I have been speaking were made in 1833, 1834, and 1835. I had previously paid attention to the same questions, and had arrived by somewhat different methods at the same conclusion with Faraday—namely, that it is in chemical action that resides the origin of the evolution of electricity in the voltaic pile. Faraday frequently alludes to my investigations in a very kind manner; and subsequently (in 1840) he wrote me a letter in which he said that, being a thorough adherent of the chemical theory, he had just attacked the question directly, as I had already done, by demonstrating that con-

tact alone, if not accompanied by chemical action, is not a source of electricity. The memoir in which he probes this question to the bottom is the last which he devoted to this department of electricity. In it, by means of a multitude of ingenious experiments, he demonstrates that the presence of an electrolyte (that is to say, of a liquid which is at once a compound and a conductor of electricity) is indispensable for the production of electricity in a voltaic couple; he varies his experiments in a thousand ways, sometimes by exhausting the number of chemical compounds employed as electrolytes, sometimes by the intervention of temperature or of other agents; and he concludes by showing by general considerations the improbability of the existence of a force of contact.

We may say that this last work, a precious supplement to the preceding ones, has rendered perfectly evident the truth of the chemical theory. This theory, foreseen by Wollaston and Fabroni, but opposed by most of the physicists of the early part of the present century, had found a powerful argument in its favor in the beautiful experiments of the elder Becquerel upon the electricity developed by chemical actions. It was then (from 1825 to 1835) that, profiting by these experiments, and seeking, on my own part, to make others of the same kind although in a slightly different direction, I published several memoirs to support and render more precise the chemical theory of the voltaic pile. But I cannot but admit that we are indebted to Faraday for having based this theory upon irrefutable proofs, not only by the great number and variety of his researches, but especially by his beautiful discovery of the definite decomposing action of the electric current—a discovery which established between the external chemical action of the voltaic pile and the chemical action which takes place in the interior of this apparatus, a relation so intimate that it is impossible not to see in the latter the cause of the former.

III.

In 1831 Faraday discovered electrical induction; it is the most important, although perhaps not the most brilliant of his discoveries. Ten years before (in 1821) he had observed a perfectly new phenomenon in the science of electrodynamics,—that science which issued complete, as we may say, from the brain of Ampère, after Œrsted's discovery. Struck by the experiments of the great French physicist upon the mutual attractions and repulsions of electrical currents and magnets, Faraday was led, by theoretical ideas which were rather disputable and not very conformable to the principles of mechanics, to assume that an electric current must turn round the pole of a magnet with a

continuous movement, and reciprocally that the pole of a magnet must in like manner turn round an electric current. He verified this double result by experiment; and Ampère soon showed its accordance with his theory, adding to it other facts of the same nature. It is not the less true that the discovery of a continuous movement of rotation due to the combined action of a magnet and an electric current was quite unforeseen, and at the same time very important; for up to that time there was no example of any such action in physics. It was a first step in the course which was to lead to the finding of a relation between mechanical movement and the molecular forces.

Arago (in 1824) was the first who directly established this relation, by his beautiful discovery of magnetism by rotation; for he showed that simple mechanical movement could render a body, in itself non-magnetic, capable of acting upon the magnet. Faraday advanced still further in 1831, by discovering that it was sufficient to bring toward, or remove from, a metallic wire forming a closed circuit another parallel wire traversed by an electric current, or simply a magnet, in order to develop in the former wire an electric current. He discovered induction—that phenomenon which so many others had sought in vain, although suspecting its existence, but which he alone had succeeded in producing.

Let us dwell for a moment upon his fundamental experiment. Two metal wires covered with silk are rolled together round a cylinder of glass or wood; the two wires are thus isolated, and have all their spires approximate and parallel. An electric current is passed into one of these wires; immediately a current is manifested in an opposite direction in the neighboring wire, the extremities of which are united by a galvanometer; but this current only lasts for a moment. The current passing through the first wire is interrupted; immediately another current is developed in the second wire, which is momentary, as in the former case, but directed in the same way as the producing current, instead of in the contrary direction. The momentariness of these two currents, and the fact of their alternately opposite directions, constitute the two important characters of this new mode of production of electricity.

Faraday did not stop at this. Starting from Ampère's idea that a magnet is only an assemblage of electric currents arranged round an axis in a manner very analogous to the circulation of an electric current through a metallic wire rolled into a coil, he tried the replacement, in his fundamental experiment, of the wire traversed by the current by a simple magnet. For this purpose he twisted a single wire instead of two into a coil round a glass or wooden tube; then he introduced a magnet into this

tube, and ascertained that at this moment a momentary current is developed in the coil of wire, and that a second, equally momentary but in an opposite direction, is developed at the moment when the magnet is withdrawn. Here, therefore, was realized that production of electricity by magnetism which Faraday had long been seeking, convinced, as he was, that, as electricity produces magnetism, magnetism in its turn must produce electricity.

Is it necessary to follow Faraday in the multiplied experiments by which he demonstrates that the electricity developed by induction possesses all the properties of voltaic electricity, and of the ordinary electricity produced by machines—that it heats fine metallic wires, gives shocks, and even produces the spark? To produce an electric spark by means of the action of a simple magnet, is one of those striking facts which give to the discovery leading to such a result a popularity, if I may venture so to express myself, which is reflected upon its author.

Faraday soon showed that terrestrial magnetism, like that of a magnet, can develop electric currents by induction in a metallic wire rolled into a coil or circle, and actuated by a movement of oscillation in a plane perpendicular to that of the magnetic meridian. He found that it was not even necessary to employ metallic wires to ascertain the influence of the terrestrial magnetism upon the production of induced currents, but that it sufficed to set a metallic disk (of copper for example) in rotation in a plane perpendicular to the direction of the inclination-needle to find that it is traversed by electric currents passing from the center to the circumference, or from the circumference to the center, according to the direction of the rotation. Still more readily does the vicinity of a magnet to a similar disk set in rotation in any plane under the influence of this magnet develop in it induced currents, the presence of which, directly ascertained, explains in a perfectly satisfactory manner the phenomena of magnetism by rotation discovered by Arago.

These currents, although difficult to perceive, must nevertheless possess considerable power, since they can drag a rather heavy magnet by the action which they exert upon it. It is probable that this power is due less to their individual intensity than to their number, which appears to be very considerable. We may cite two examples which prove in a striking manner the energy which this mode of production of induced currents may acquire. The first is furnished by a curious experiment of Faraday's, in which, on causing a cubical mass of copper suspended by a thread between the poles of an unmagnetized electromagnet to turn upon itself, he saw this mass stop suddenly the moment he magnetized the electromagnet, in consequence

of the magnetic action exerted by the currents which induction had set up in the copper. We find the second example in the fact observed by Foucault, of the sudden stoppage which is likewise experienced by a thick disk of copper set in rotation between the poles of an electromagnet the moment the latter is magnetized. This stoppage is such that it can only be surmounted by a considerable effort, and the disk itself becomes very strongly heated if the rotation be continued in spite of the resistance it meets with. In order that such a heating effect should be produced in a mass of such considerable size, and that we should experience an attractive action so strong on the part of the electromagnet, the induced currents thus produced must be of very great power—a power which they owe essentially to the excessive rapidity of the movement generating them.

I shall not follow Faraday through all his works upon induction which accompanied his fundamental discovery. I shall only refer to the fact that in 1834 he discovered a new important fact—namely, the production of an induced current in the very wire that conducted the inductive current, and which takes place at first at the moment when the latter current begins to circulate, and then at that when it ceases passing. If this wire is rolled in a coil round a cylinder of soft iron, the effect produced acquires great intensity by the fact of the alternate magnetization and demagnetization of the iron which accompanies the passage and interruption of the current in the wire. We all know the advantage that has been taken of this combination in the construction of very powerful apparatus. We also know how, from one improvement to another, we have come to find in induction, and consequently in the simple mechanical movement which gives birth to it, the most simple and economical principle for obtaining electricity, especially with regard to its application to therapeutics and illumination.

The discovery of electrodynamical induction (that is to say, the production of a current by the influence of an exterior current) led Faraday to examine more closely than had previously been done into the phenomenon of statical induction—that is to say, the development at a distance of tension-electricity in an isolated conductor by the influence of an electrized body. He ascertained, what no one had previously suspected, that the nature of the body interposed between the source of electricity and the conductor submitted to the action of this source had a great influence upon the effect produced—that, of the various bodies, some facilitated the development of electricity at a distance, whilst others completely stopped it. He named the former *dielectrics;* and he proved that these dielectrics, which are essentially resins, sulphur, shellac, oils of turpentine and

naphtha, &c., enjoy this property of transmitting electricity by influence in different degrees, whilst there is not in this respect any difference between the gases, which have the same dielectric power whatever their nature or their density may be. On the other hand, none of the metals are dielectric; they are subject to the electrical influence but do not transmit it.

From the investigation which we have just summarized, Faraday drew the conclusion that induction does not take place at a distance, but that it is affected by the intermediation of the particles interposed between the inductor and the inducted body. He assumed that these particles are polarized one after the other, which M. Matteucci afterward demonstrated directly by experiment; that consequently the mode of propagation of electricity is the same in insulating as in conducting bodies; and that the various substances only differ from each other by the greater or less facility or rapidity with which this polarization, necessary for the transmission of electricity, takes place in them. Then, passing from this to analysis of the different modes in which electrical discharges take place, some obscure, others luminous, some electrolytic (that is to say, accompanied by the chemical decomposition of the conducting body), others disruptive (that is to say, effected by the mechanical disjunction of the particles of the interposed substance), he applied himself more particularly to the study of the various forms displayed by the electric spark in more or less rarefied gases. I should never have done if I were to attempt to explain all the experiments which he made to elucidate these different points and to arrive at an idea of the actual nature of the electric current. The identity of the current, whatever may be its origin,—that its production is due to polar forces which may exert a transverse action, as is the case in electrodynamical phenomena,—that these polar forces emanate from contiguous particles; such are the principles which Faraday endeavored to establish as the consequences of his experimental researches, at the same time that he rejected the idea of actions at a distance, referring all electrical manifestations to the presence of a ponderable matter.

Whether or not we completely admit all Faraday's ideas, it is impossible not to acknowledge the immense advance which he caused the theories of electricity to make, either by demonstrating by experiment the falsity of certain conceptions generally accepted up to his time, or by opening up perfectly new points of view as to the actual nature of electrical phenomena. We have just had the proof of this in the consequences to which he was led by his investigations on statical

induction. His discoveries in electrodynamical induction have had still more important consequences, by introducing the notion of mechanical movement into the essence of electrical movement, and thus enabling Weber to combine, in an equally ingenious and satisfactory manner, the mechanical phenomena of electrodynamics, discovered by Ampère, with the electrical phenomena due to mechanical movement, discovered by Faraday.

Ampère and Faraday,—two names which will always be united by the intimate relation of their works to the history of the science of electricity, in which they have opened such new and vast horizons ; and yet minds as dissimilar in their mode of proceeding as similar in the power of their genius. Both eminently endowed with that faculty of divination which generates great discoveries, but one of them, Faraday, arriving at them by impression, by a kind of instinct which never deceived him,—the other, Ampère, advancing with a more certain step, having as his instrument those calculations which he handled with such remarkable ability, and thus arriving at results which he hardly required experiment to confirm, so certain was he that this would not contradict him.

IV.

I now pass to the last great series of Faraday's works. I have said, and, I think, proved, that induction was the most important of his discoveries ; I must now say that the action of magnetism and electricity upon light was the most brilliant. Often the attempt had been made to see whether magnetism and electricity exerted any direct influence upon light ; but these attempts had always failed. Investigators had operated upon luminous rays traveling in the air or in liquids, and endeavored to act upon them, sometimes by strong magnets, sometimes by electric currents or by statical electricity ; but these attempts had led to nothing, absolutely nothing. All these negative investigations have never been published, but they have nevertheless been made.

Guided by theoretical considerations upon the mutual correlation of the forces of nature, Faraday, after many fruitless attempts, succeeded in finding the connection which exists between light and the magnetic and electric forces. Instead of taking an ordinary ray, he operated with a polarized ray ; instead of acting directly upon this ray by means of a magnet, he submits it to the influence of magnetism whilst it is traversing a glass prism in the direction of its length. This prism, terminated by two square and parallel bases, the surfaces of which are well polished, and which are those by which the po-

larized ray penetrates and issues from the prism, is placed between the poles of an electromagnet in such a manner that its length, and, consequently, the direction of the transmitted ray are parallel to the line joining the magnetic poles. Lastly, the polarized ray on issuing from the glass prism only reaches the eye after passing through a Nicol's prism which serves as an analyzer. It is also by traversing a Nicol's prism before penetrating into the glass prism that the ray of light is polarized; but this may be effected in any other manner.

It is well known that by turning the analyzing prism to a certain angle the polarized ray is extinguished, in such a manner that the brilliant spot is replaced by a black spot. If, after this operation has been effected, a strong electric current is passed through the wire surrounding the electromagnet, the black spot disappears and the bright one again makes its appearance. Then by turning the analyzing prism a little further in the same direction, the luminous ray is again extinguished, but this extinction ceases as soon as the magnetic action is suppressed by the interruption of the current which magnetized the electromagnet. The action of magnetism therefore consists simply in causing the plane of polarization to turn by a certain angle, and to give artificially to the glass, while it is under the magnetic influence, a property which certain substances, such as quartz and essence of turpentine, possess naturally.

Any transparent substance, except gases, may serve, although in different degrees, as the *medium* for magnetism to act upon the polarized ray. But that by means of which this influence is best manifested is the yellowish heavy glass (borosilicate of lead) which Faraday obtained in his experimental researches upon the fabrication of glass for optical purposes. He happened to have at hand several specimens of this glass; and it was by using one of these for performing the experiment just described that he discovered the magnetic rotation of the plane of polarization, a phenomenon which would probably have escaped him if he had made use of ordinary glass at first starting. Thus the long and painful labors to which he had formerly devoted himself without any great success, in order to discover a glass fitted for the fabrication of lenses, were not lost to science, since they facilitated his enriching it with one of his finest discoveries.

Let us now study the new phenomenon a little more closely, so as better to show all its importance. Some substances we have said, naturally possess the property of causing the plane of polarization of a polarized ray traversing them to rotate through a larger or smaller angle; some cause it to turn to

the right, and others to the left of the observer. The discovery of Faraday was that the influence of magnetism or of electric currents develop this same property in nearly all transparent substances, but with this difference, that the direction of rotation of the plane of polarization depends only upon the position of the magnetic poles, or the direction of the currents with relation to the transparent substance. The law is that if the north pole of the electromagnet is placed on the same side as the observer who receives the ray into his eye, and consequently the south pole on the side by which the polarized ray enters into the substance, the rotation of the plane of polarization takes place, to the observer, from left to right. It takes place from right to left if the direction of the current, and consequently that of the magnetization, be changed. The action of the magnet may be replaced by that of a coil in the axis of which the transparent substance is placed. In this case, again, the rotation of the plane of polarization is very well observed when a rather strong current is transmitted through the wire of the coil ; and the direction of the rotation is always the same as that of the current.

Thus, whilst in substances naturally endowed with circular polarization the rotation of the plane of polarization always takes place, according to the nature of the substance, either to the right or left of the observer, in Faraday's experiment the direction of this rotation only depends upon the direction of electric currents or the relative position of the magnetic poles, since it is completely independent of the position of the observer. These two kinds of action are therefore not identical, and we cannot say that by the influence of the magnet or of electricity we produce in all transparent bodies exactly the same property that certain substances naturally possess. Faraday well shows this difference by an experiment which consists in producing by an ingenious artifice the internal reflection of the polarized ray upon the extreme surfaces of the prism; this may be done once or several times before the ray is allowed to escape, and doubles, triples, or quadruples the angle of rotation of the plane of polarization, according as the ray is reflected once, twice, or three times. But when, instead of the magnetic, we have to do with the natural rotatory polarization, the result is quite different, the return of the reflected ray neutralizing the effect which the direct ray had undergone while traveling in an opposite direction. In this case the angle of rotation of the plane of polarization reflected twice, and which consequently has three times traversed the transparent substance, is no greater than that of a ray which has only traversed it once.

The general phenomenon so unexpectedly discovered by Faraday has hitherto remained unexplained, notwithstanding many investigations, and especially the persevering and remarkable researches of Mr. Verdet.

It has not even been possible to connect it with some other property of bodies, although each substance has its specific magnetic rotatory power. Faraday, however, drew from it a general consequence which led him to another discovery, namely, that magnetism acts upon all bodies, since all transparent bodies may be modified under its influence sufficiently to acquire, in different degrees indeed, a power which they do not possess of themselves. The discovery to which I have just allluded is that as the magnet acts by attraction upon magnetic bodies, it acts also by repulsion upon all other bodies in nature. From this it results that whilst a rod of iron, or of some other magnetic substance, suspended between the poles of an electromagnet, places itself *axially* (that is to say, parallel to the line which joins the poles), a prism of heavy glass (the same, for example, which served for the experiments on light) places itself equatorially (that is to say, transversely to this line). A rod of bismuth is in the same case; and this metal and heavy glass are the substances on which this repulsive action of the magnet is most distinctly exerted; but all bodies in nature which are not magnetic (and these are by far the most numerous) present the same property, although in various degrees. In this way Faraday comes to class all bodies under two heads:—those which are magnetic, or *paramagnetic*, as he calls them, such as iron, nickel, &c.; and those which are *diamagnetic*, such as bismuth, antimony, heavy glass, &c. The character of the former is to be attracted by the magnet, that of the latter to be repelled by it. It is true that this repulsion, to become sensible, requires an enormous magnetic power, even in the case of bodies of which the diamagnetism is most strongly marked, whilst a very weak magnet is sufficient to betray its action upon the magnetic bodies, such as iron, steel, nickel, &c.

It therefore required very powerful means, such as Faraday employed, for the discovery of diamagnetism. Nevertheless a distinguished amateur in science, Mr. Lebaillif of Paris, had shown, as early as 1828, that a fragment of bismuth or antimony very evidently repels a delicately suspended magnetized needle when brought as near as possible to one of the poles of the needle but without touching it. Mr. Faraday was ignorant of this circumstance when he published his first work on diamagnetism. I immediately informed him of it, at the same time indicating the journal in which I had published Mr. Le-

baillif's experiment, which I had witnessed at the time. He accepted my reclamation in the most amicable manner, and at once, with his usual good faith, recognized the priority of Mr. Lebaillif with regard to bismuth and antimony.

In the numerous researches which Faraday devoted (from 1845 to 1855) to diamagnetism and at the same time to magnetism, there are some important points which I must indicate. He discovered the remarkable influence exerted upon this kind of properties by the molecular constitution of bodies, and especially by crystallization. He showed, for example, that a crystallized lamina of bismuth or antimony can place itself axially between the poles of an electromagnet like a magnetic body, as well as equatorially, and that the position which it takes depends on the manner in which it is suspended relatively to the direction of its cleavage. He endeavored to investigate the force which comes into play in facts of this order, which he names *magnetocrystalline* force ; whilst Plücker, on his part, widened its field by his beautiful and numerous researches on the manners in which crystals place themselves between the poles of an electromagnet ; and Tyndall, the worthy successor of Faraday at the Royal Institution, by his ingenious experiments analyzed the phenomenon in its generality and succeeded in connecting it, in a perfectly satisfactory manner, with the laws which govern magnetism and diamagnetism. Subsequently Tyndall succeeded also in demonstrating, by a decisive experiment, that diamagnetism, like magnetism, is due to a polarity caused by the influence of the magnet in the diamagnetic body, but with this difference, that, instead of opposite poles, homonymous poles are developed by the poles of the magnet. Thus fell to the ground all the other more or less rash attempts at explanation which had been given of diamagnetism.

Another point which deserves attention is the investigation which Faraday made of the magnetism and diamagnetism of gases. He arrived at this curious result (observed likewise by Edmond Becquerel at the same time), that of all gases oxygen alone is magnetic, and this in a very marked degree, while all the other gases are diamagnetic. Considering the great part taken by oxygen in the composition of our atmosphere, he attempted to explain, by the magnetic properties of this gas combined with variations of temperature, the phenomenon of the diurnal variations of the magnetic needle which he traced over all parts of the surface of the globe. It is impossible for us not to regret a little the considerable time which he devoted to this investigation, especially as it appears to us very probable that it is not in the action of the atmosphere, but much rather

in that of the earth itself, or perhaps even in that of the sun, that we must seek the cause of all the phenomena presented by the magnetic needle.

Lastly, a third point remains to be noticed, namely, that which relates to the investigation of the magnetic field and of what Faraday denominates the lines of magnetic force. According to him, as we have already had occasion to remark, there is no such thing as action at a distance; consequently the magnetic field (that is to say, the space included between two approximated magnetic poles, such as those of a horseshoe magnet) is a medium from which, in every one of its points, forces emanate, the distribution and direction of which are indicated by the very regular arrangement affected by fine iron-filings placed in this space. The lines which he calls lines of magnetic force thus become visible and even tangible. But they exist none the less even when we cannot see them, and it is the displacements or modifications which they experience by the presence of a ponderable body in the medium in which they occur that give rise to all the remarkable effects of which the magnetic field is the scene. Such is, in a few words, Faraday's view upon this particular question.

We pass in silence over a multitude of interesting details upon diamagnetic polarity, upon the distinction to be set up between magnetic and diamagnetic bodies, and upon the possible relation between gravity and electricity. In 1850 Faraday reverted to this question, which he had previously attempted but without success. We see that it is with regret that he is obliged to relinquish the discovery of this relation, which he had twice sought after; but with his usual good faith he admits that, although convinced that it exists, he was unable to find any fact to establish it. If experiment, which he knew so well how to employ constantly, gave him a negative response, would not this be because his point of view was not correct? and did not his error arise from his forming too vague ideas as to the transformation of forces, not taking sufficiently into account that it is the work effected by the force, and not the force itself, that must be considered in questions of this kind?

V.

We have passed in review the principal labors of Faraday; and it only remains for us, in order to complete this notice, to endeavor to form an idea of the special character of these labors, and of the influence which they have exerted on the progress of science.

The first character that strikes us is their number. What Faraday published in the form of memoirs from 1820 to 1855

is incredible. And what would it have been if, side by side with the multitude of experiments which he has made known, we placed in a parallel series those which he never published? It is true that if he has left them buried in his journal, it is because they gave him negative results; but from how many fruitless essays and erroneous attempts he would have preserved scientific men if he had not been so discreet!

A second character is the exactitude of the results obtained: I do not think that Faraday has once been caught in a mistake; so precise and conscientious was his mode of experimenting and observing. It must be admitted that in him the hand marvellously seconded the head; he was of remarkable dexterity, and possessed a practical talent, rare and precious in men of science, which enabled him, when necessary, to construct and modify his apparatus for himself, with the view of attaining with more certainty the desired result.

A third character, of quite a different kind and of much greater value, is the originality of the works of Faraday. A disciple of Davy, he undoubtedly shows traces of the school from which he came, especially in the choice of the subjects of which he treats; but he does not blindly follow either the method or the steps of his master, and, soon quitting the beaten track, he strikes out a path for himself. What is this path? I shall be asked. This is not easy to say; but I will nevertheless attempt it.

At the commencement of the present century, thanks to the important works of which it had been the subject, the science of physics had acquired a character of precision and clearness which seemed almost to make of it a mathematical science. The fine treatise, in four volumes, on Experimental and Mathematical Physics, published in 1816 by M. Biot, gives the most correct and complete idea of the point at which this science had arrived. To the confusion which still reigned in the middle of the eighteenth century between the various departments of the science, to the ignorance which then still prevailed upon a great number of these departments, succeeded a clear and substantial analysis of all the phenomena, brought under simple and rigorous laws. Heat, light, electricity, and magnetism were regarded in it as so many distinct agents, having their special properties and obeying their own laws. Calculation was admirably fitted to these clear and precise conceptions; hence we find it greatly used, as witness the very title of M. Biot's treatise.

The great discovery of Œrsted (in 1820), upon the relations existing between electricity and magnetism, began to diminish confidence in this mode of considering the phenomena, a con-

fidence which was already a good deal shaken by the researches of Fresnel and Arago upon light. The breach once opened, the fortress was soon entered; and among the most intrepid assailants Faraday figures in the front rank. By his researches on the condensation of gases, he shows that there is nothing absolute in the laws of Mariotte and Gay-Lussac and in the distinction so generally accepted between vapors and permanent gases. By his investigations upon voltaic electricity, he establishes between chemical affinity and the production of electricity a relation so intimate that it seems as if the one was only a form of the other. By his discovery of induction, he brings in mechanical movement as an important element in the production of electrical phenomena. By his experiments on the influence of the magnet and of electricity on polarized light, and by those which were the consequence of it, he opens to science a new path which no one had foreseen. He succeeds thus in establishing between the natural agents which we name light, heat, electricity, magnetism, chemical affinity, and molecular attraction, such intimate relations, such a connection, that it is impossible not to think that we shall one day succeed in demonstrating that they are only different forms of the same agent. No doubt he is not the only one that has followed this path. Many others have brought in their contingent to this work of demolition and reconstruction; but he was one of the first, most active, and most persevering. Therefore his works, I have no doubt, will always be regarded as corner-stones in the new edifice which we are now endeavoring to construct.

I designedly say, *which we are endeavoring to construct;* for we must carefully avoid thinking that it is already constructed. Since the fine discovery of the mechanical equivalent of heat, it seems as if everything had been said and everything were easily explained by means simply of a ponderable matter, an imponderable æther, and a mechanical impulse. Vulgarizers of science, more anxious to produce an effect than to remain faithful to scientific truth, proclaim a molecular system of the world destined to form a pendant to the *Mécanique Céleste* of Laplace. According to them, nothing is more simple, nothing clearer; attraction itself, which has been the object of the study of so many superior minds, is merely the effect of an impulse easy to understand. A dangerous illusion! which, if it succeeded in propagating itself, would be as fatal to the true progress of science as opposed to its useful diffusion; for it is especially upon those who take to themselves the high mission of popularizing science that it is imperiously incumbent to spread none but correct and well founded ideas.

Let us not, however, exaggerate anything, or refuse to recognize in the too positive ideas which we have just combated that portion of truth which they may contain. With this purpose let us try, in conclusion, to lay down in few words the point at which, in our opinion, in the present state of science the important question of the unity of forces has arrived.

After having for a long time arrested the progress of science by abstract and general considerations upon the phenomena of nature, the philosophers finished by adopting, with Galileo, the experimental method, the only one that can lead with certainty to the discovery of the truth. A rigorous and profound analysis, placed at the service of this method, furnished certain and fundamental results. Reverting to a synthetic phase, many superior minds now seek by means of these tediously and painfully collected materials to reconstruct the edifice of which the raising was formerly attempted in vain. No doubt science has thus entered upon a fertile course, but only on condition of advancing with sure and consequently with slow steps. We speak of the unity of force, and of the transformation of forces one into the other; but do we know what are forces? do we know their nature? We have certainly proved transformations of movement, and shown that one work may change into another work, mechanical motion into heat, and heat into mechanical motion; these are, without doubt, the most important points gained by science, and enable us to get a glimpse of the existence of a single cause manifesting itself in various forms. But it is a long way from this to the discovery of this cause, this single force. Shall we some day arrive at it? It is possible and even probable; and in this case the name and works of Faraday will always remain associated with one of the greatest problems which the human mind can entertain.

ART. XIX.—*Contributions to Chemistry from the Laboratory of the Lawrence Scientific School.*—No. 5. *On a new process in Mineral Analysis;* by FRANK WIGGLESWORTH CLARKE, S.B.

IN the course of some experiments upon the alkaline fluorids, I found that the most refractory minerals, such as emery, chromite, tinstone, rutile, &c., were completely and easily resolved by fusion with a mixture of fluorid of sodium and bisulphate of potassa.

The operation is performed as follows. One part of the

finely pulverized mineral is mixed in a platinum crucible with three parts of fluorid of sodium, and upon the top of this mixture are placed twelve parts of bisulphate of potash, which may be either in powder or in small lumps.

Upon heating, the mixture boils up strongly, and after a while settles into a clear, tranquil fusion. The boiling is chiefly owing to the action of the reagents upon the mineral, and not, as might be supposed, merely to the influence of the bisulphate upon the fluorid. This is shown by the fact that, whenever the reagents are heated together without minerals, although some boiling takes place, the addition of a little powdered chromite or iron ore fully doubles the violence of the action.

In quantitative analyses, it is necessary to keep the crucible closely covered in order to avoid loss from spattering; and to heat carefully, so that the mass may not boil over. The bisulphate should never be mixed with the fluorid and mineral, because a portion of the assay is then apt to escape action, being left on the sides of the crucible by the boiling of the mass; but should be placed upon the top of the mixture as above directed, as then the decomposition is complete. The mass obtained by this fusion, is, in the case of some minerals, completely soluble in water. In other cases, basic salts are formed, which, although insoluble in water, dissolve readily in hydrochloric acid. Almost all of the latter class may be rendered soluble in water, however, by the following process. The fused mass, after cooling, without removal from the crucible, is treated with a small quantity of strong sulphuric acid, and again fused. The mass thus obtained is entirely soluble in water. There are exceptions to this rule, however.

I will now describe the results I have obtained with various minerals, and for the sake of brevity, will speak of the fusion with bisulphate and fluorid as fusion No. 1, and the subsequent treatment with sulphuric acid, as fusion No. 2.

Tinstone is completely resolved, giving a white mass almost entirely soluble in *cold* water. A very small quantity of stannic acid remains undissolved, however, but by first treating the mass with hydrochloric acid and then adding water, complete solution is ensured. By rendering the solution *nearly* neutral, taking care, however, to leave a slight excess of sulphuric acid, then reducing the iron by sulphuretted hydrogen or hyposulphite of soda, and then boiling for sometime, all the tin is precipitated in the form of stannic acid. If the ore contains tungstic, niobic, or tantalic acids, these also will be completely precipitated. A second fusion is of no advantage with tinstone. This is the only mineral which I have yet tested,

which was not completely resolved by this process, over an ordinary Bunsen's gas burner. This substance required the heat of a blast lamp.

Wolfram is entirely decomposed, affording a pale yellowish mass, partly soluble in water. Hydrochloric acid dissolves a part of the residue, but a white powder, probably the hydrate of tungstic acid, remains unattacked. Fusion No. 2 possesses no advantages with wolfram. I should not recommend the use of this process for the analysis of wolfram, as it is so readily decomposed by nitric acid. I made the experiment, however, because as wolfram is so often mixed with tinstone, it seemed to me necessary to know what the reaction would be.

Rutile.—This mineral is rapidly and easily resolved, giving a mass, when cooled, of a yellowish white color, and, as I obtained it, presenting a beautiful mottled appearance, being crystalline in structure, and in some parts nearly transparent, but opaque in others. This mass dissolved entirely in *cold* water, rendering a second fusion unnecessary. From this solution all the titanic acid may be thrown down by boiling.

Niobite is decomposed with the greatest ease. The mass is pale yellow, and partially soluble in cold water, but a small quantity of niobic acid remains undissolved, even in hydrochloric acid. But it is always best to treat with hydrochloric acid, in order to dissolve any basic sulphates of iron that may be formed. Upon boiling the filtered solution, niobic acid is precipitated.

Fusion No. 2.—A larger proportion of the mass is soluble in water than with the first fusion, as no basic salts of iron are present.

Ilmenite is completely decomposed, giving a mass closely resembling that obtained from rutile. *Cold water* dissolves a large part of this, but leaves some basic salts which dissolve readily in hydrochloric acid.

Fusion No. 2.—The resulting mass dissolves completely in *cold* water, and by boiling the solution, the titanic acid can be precipitated.

Chromic iron ore is decomposed very easily. In one case in which I timed the operation, *the fusion was complete in less than three minutes from the time I began to heat*, and that over an ordinary Bunsen's gas burner. The cooled mass is light green, partly soluble in water alone, and entirely soluble in hydrochloric acid.

Fusion No. 2.—The mass possesses a deeper green color than that obtained by the first fusion, and a larger proportion of it dissolves in water. In every fusion that I have yet made of chromite, however, a small quantity of basic salts was formed, requiring treatment with hydrochloric acid.

Emery is rapidly and easily resolved. The mass contains basic compounds that require hydrochloric acid for complete solution; although water dissolves a large proportion of it.

Fusion No. 2.—The mass thus obtained is entirely soluble in water.

Hematite.—(An exceedingly hard specular ore from the Tilden mine, Lake Superior). It was completely resolved, giving a mass partially soluble in water, but dissolving entirely in hydrochloric acid.

Fusion No. 2.—The mass dissolves completely in water.

Limonite and *magnetite* behave exactly like hematite.

Zircon is entirely decomposed. All its silica is converted into the gaseous fluorid of silicon, and driven off. The mass obtained resembled that given by rutile. It dissolved almost entirely in cold water, but the solution speedily became turbid and deposited a white precipitate, which was either Zirconia or some basic salt of that oxyd. By first digesting the mass with a little strong hydrochloric acid and afterward adding water, the whole went into solution.

Fusion No. 2, afforded a mass of a beautiful waxy luster, which was completely soluble in water.

Kyanite is entirely resolved, and, like zircon, freed from silica. The white mass contained basic compounds, and consequently required hydrochloric acid for complete solution. A second fusion gave a mass entirely soluble in water.

Orthite is completely decomposed, and deprived of silica. The mass was white, and dissolved partly in water. The insoluble residue contained basic salts and some sulphate of lime, and hydrochloric acid dissolved all but the latter.

Fusion No. 2.—With the exception of a little sulphate of lime, the mass dissolved in water.

Quartz sand.—I subjected some of this substance to fusion with the mixture of bisulphate of potash and fluorid of sodium, in order to ascertain to what extent silica would be converted into fluorid of silicon. The fusion took place very easily, giving a white mass which dissolved almost entirely in water. Only a very small trace of insoluble residue remained, probably not more than one tenth of one per cent of the quantity of sand taken. It is very probable that more careful treatment would get rid of even that small amount. After this it seems impossible to doubt that any and all silicates would be decomposed and freed from silica by this process. A convenient method is thus afforded for the estimation of bases in silicates, bearing in mind, however, that a second fusion with sulphuric acid will in most cases be necessary.

As far as concerns the complete resolution of any mineral, pure, finely pulverized cryolite may be substituted for fluorid of sodium. It labors under the disadvantages, however, of introducing alumina into the mass obtained, but nevertheless it can be advantageously employed in cases where the introduction of alumina is a matter of indifference, as in the technical analyses of tinstone and chromite, and the estimation of iron in ores, slags, and cinders. The perfectly white translucent specimens of cryolite should be chosen for this purpose.

Either the bisulphate of potash or of soda may be used, and although neither seems to possess any advantage in point of thoroughness, the potassa salt appears to be the most readily fusible, and is therefore to be preferred.

The following are the advantages that I claim for this process.

First. Speed. Among the different minerals upon which I have tested the action of this mixture, I have found no case where I was obliged to heat longer than five minutes, and many fusions are complete in three, and even less.

When bisulphate of potash *alone* is used for a similar purpose, it is usually necessary to heat for from half to three-quarters of an hour; and even then in the cases of emery, chromite, and some other minerals, it is almost impossible to obtain absolutely complete resolution.

By my process, even when the second fusion with sulphuric acid is necessary, not more than twenty minutes should be consumed in both fusions and the time for cooling between them.

Second. In every case except tinstone that has come under my observation, in my process, an ordinary Bunsen's burner may be used as the source of heat; whereas, by most other methods, a blast lamp is necessary.

Third. When bisulphate of potash *alone* is used for the decomposition of chromite, etc., it is necessary that the mineral should be reduced to extremely fine powder; but, when the mixture of bisulphate and fluorid is employed, although the mineral should be in fine powder, such an extreme state of subdivision is by no means required, and thus much labor is saved.

Fourth. In all cases when this mixture is used, the resolution of the mineral is absolutely perfect. Furthermore, all the silica is got rid of at once, and all the bases present are converted into sulphates. This last is a great advantage whenever iron is to be determined volumetrically by means of hypermanganate of potassa solution. As far as thoroughness of action is concerned, or generality of application, I can claim no advantage for my process over the use of acid fluorid of potassium; but the last named reagent is difficult to prepare, and when prepared, cannot be preserved in glass vessels.

Fluorid of sodium is subject to neither of these disadvantages, and in the mixture with bisulphate of potassa, has slight advantages as regards rapidity of action.

It may not be out of place for me to mention here a fact connected with the use of fluorid of ammonium for decomposing silicates, as described by Rose. After fusing the mineral with the fluorid, he directs treatment with sulphuric acid, for the purpose of converting the bases into sulphates. I find that if, in the first place, sulphate of ammonia is mixed in excess with the fluorid, all the bases are directly converted into sulphates, thereby obviating the necessity of treatment with sulphuric acid.

This method *can* be used for the purpose of determining alkalies in silicates, but is far inferior to J. Lawrence Smith's process.

Technical determination of chromium in chromite.—After fusion with cryolite and bisulphate of potash as previously directed, the mass is to be treated with a little strong hydrochloric acid, and allowed to digest for about ten minutes. Then, upon boiling with water, the whole dissolves. The solution should then be neutralized, acetate of soda added, and the chromium oxydized to chromic acid by a current of chlorine gas, or by boiling with hypochlorite of soda solution. The chromium may then be separated from other substances as directed in Prof. Gibbs's paper in this Journal for Jan., 1865. When chromite is fused with bisulphate of potash and cryolite, and saltpetre is added to the mass, as soon as clear fusion is obtained, the chromium is nearly all oxydized to chromic acid. If the mass be boiled with a solution of carbonate of soda and the liquid filtered, a filtrate is obtained which contains nearly all, but not quite all the chromium as alkaline chromates, free from iron or alumina; but, invariably, the residue upon the filter contains traces of chromium. When chromite is fused with the acid fluorid of potassium, a part of the chromium is usually oxydized to chromic acid by the oxygen of the air; and in one case that came under my observation, when I came to heat the resulting mass with sulphuric acid, red fumes were given off, which were probably the so-called terfluorid of chromium.

Technical estimation of iron in ores, slags, and cinders.—After fusion with cryolite and bisulphate of potash, the mode of treatment varies according to the method it is desired to use for determination of the iron. If Penny's process of estimating iron volumetrically with bichromate of potassa, or the ordinary method of precipitation with ammonia is to be employed, the mass may be treated with hydrochloric acid, and thus brought immediately into solution. If, on the contrary,

the iron is to be determined volumetrically by means of hypermanganate of potassa, the use of hydrochloric acid is interdicted, and it becomes necessary to fuse again with sulphuric acid and dissolve in water. When volumetric processes are used, the whole determination of the iron, from the time the assay is placed in the crucible, should take less than an hour to perform.

This process has a great advantage over all others, in the examination of ores, slags, and cinders containing iron, both as regards speed and convenience. A perfectly clear solution is immediately obtained without filtering, all the silica is got rid of, and it is only necessary to reduce the iron with hydrogen, and then to titrate. If in an iron ore it is desired to determine also titanic acid and manganese, it is best to make the subsequent fusion with sulphuric acid. The clear solution obtained is diluted to a known quantity, and by means of a graduated pipette divided into several portions. In one part the iron may be reduced and determined volumetrically; in another, the titanic acid thrown down by boiling. In still another portion, the iron, alumina, and titanic acid may be thrown down by boiling with acetate of soda, and in the filtrate, the manganese may be precipitated by a current of chlorine gas, or by boiling with hypochlorite of soda. If fluorid of sodium be used instead of cryolite in the fusion, lime and magnesia also may be estimated. For determining silica, phosphorus, and sulphur, other methods must be employed.

Preparation of fluorid of sodium.—When pure finely pulverized cryolite is boiled with caustic soda solution, it is, as is well known, decomposed. Fluorid of sodium is deposited as a jelly-like mass at the bottom of the vessel, and the supernatant liquid contains aluminate of soda, a little fluorid of sodium, and the excess of soda. The deposit of fluorid is to be washed with cold water until the washings have no longer an alkaline reaction; and is afterward purified by repeated solution and evaporation. It is so difficultly soluble and crystallizes so badly, that this last is a matter of some difficulty; but the first part of the process is so very easy that the crude fluorid of sodium can be prepared by the hundred weight without much trouble. Iron vessels are suitable for the operation, but must be very clean and free from rust. If caustic potassa be substituted for soda, the deposit of fluorid of sodium is smaller, and the supernatant solution contains aluminate of potassa, fluorid of potassium, and a little fluorid of sodium.

Possibly the fluorid of potassium might be prepared in a state of purity from this solution, but it is extremely problematical whether this could be done economically. When pure cryolite

is fused with carbonate of soda, and the fused mass powdered and treated with water, fluorid of sodium is dissolved out. This method, however, cannot compare with the first for convenience and economy.

It may not be altogether out of place to remark in this connection, that I find that when fluorid of sodium is heated with sulphate of ammonia, fluorid of ammonium is formed and sublimes. Possibly this may be turned to advantage, although I have made no experiments upon obtaining fluorid of ammonium in quantity by this process.

Before closing this paper I also wish to state that I made numerous experiments with a view toward applying the mixture of fluorid of sodium and bisulphate of potash to the estimation of both oxyds of iron when they occur in minerals; but I met with no success.

During the course of the experiments described in this paper, I have been largely indebted to Dr. Wolcott Gibbs for many very valuable suggestions and much excellent advice which aided me exceedingly.

Art. XX.—*Reëxamination of the localities of Human Antiquities at Abbeville, Amiens, and Villeneuve;* by E. Andrews, A.M., M.D., Professor of Surgery in Chicago Medical College.

During a recent sojourn in Europe, I relieved the arduousness of other duties by an occasional examination of famous geological localities. During these visits, some facts fell under my notice, which I have not seen mentioned in European works, and others forced upon my mind totally different inferences from those usually drawn. Among the localities visited were Amiens, Abbeville, and other points of the valley of the river Somme in France, in whose gravel beds are found flint hatchets and the bones of Man in connection with the bones of the *Elephas primigenius*, the *Rhinoceros tichorhinus*, and other extinct animals. The river Somme is a small stream, apparently about fifty feet wide, meandering along the flat floor of an ancient watercourse of much greater dimensions. The valley is about a mile and a half in breadth from summit to summit of the bluffs, and not far from two hundred feet in depth. In the lower part of its course, it is purely a valley of erosion excavated in soft chalk; but above Amiens it expands into wide irregular basins, which are apparently the natural undulations of the surface, which existed before the land rose from the sea,

and which are now connected by valleys of erosion extending from one to another. Upon the floor of the valley and extending far up the slopes of the sides in many places are the famous gravel beds, which sometimes attain a thickness of 20 feet. Upon the lowland gravels rests a bed of peat about twenty-six feet thick. The discoveries in this valley have mostly been made by Mr. Boucher de Perthes, of Abbeville, who appears to have used great care to avoid mistakes, and to whose courtesy I am indebted for much valuable information. Boucher de Perthes finds in the gravels, as I have already said, the bones of the *Elephas primigenius*, the *Rhinoceros tichorhinus*, and other extinct animals in such connection with the bones of men, rude flint hatchets and other human relics, as to indicate that these great extinct pachyderms have been cotemporaneous with Man. They seem, however, to have become extinct at the time of the gravel deposit, for they are not found in the peat above. Boucher de Perthes inclines to the idea that the gravels were the product of diluvial epoch, whose disturbances exterminated these animals.

The European geologists frequently speak of the flint implements of the gravel as found in the "drift." American readers may need to be reminded that the gravel of the Somme is not genuine glacial drift, such as bears that name in America, but simply a river deposit of more recent date. It is restricted exclusively to the valley, and appears much like the gravel beds flanking the river valleys of the western states, which also contain the bones of elephants, and are invariably found above the true drift.

The question of the antiquity of these relics is one of great interest, for conceding their genuineness, of which there is little doubt, it is necessary to suppose that the *Elephas primigenius* has lived later, or Man earlier than is usually believed. The answer to the problem is sought by estimating first, the probable time consumed in the deposit of the gravel, and secondly, of the peat overlying it.

Sir John Lubbock believes that the gravel was an extremely slow product of deposition, occupying untold ages in its formation. He contends that it was gradually formed by the river, while it was slowly excavating the valley from the chalk, and that the stream at that time had but little greater annual quantity of water than it has now; but that its flow was less even, more of it being concentrated into the spring floods.

In the gravel pits near Amiens, I observed some facts which have an important bearing upon the question of time. There is evidence there that at the time the deposit was formed, blocks of ice, or of mixed ice and frozen gravel, three or four feet in

diameter, were laid down in the strata, and that these blocks were completely covered and had other strata laid above them, before they had time to melt. There is also proof that the river, which did this work, had a volume not less than 1000 times that of the ordinary summer stream of the Somme. The facts are these: the mass of the upper gravels consists of chalk flints mixed with angular fragments, powder of crushed chalk and rolled chalk pebbles, the whole being of a light gray tint. Above this there is a stratum of gravelly clay of a blackish brown color, a few inches in thickness. Over this is gravel of a lighter brown, and above that, next to the soil, there is about 18 inches of a brown clay used for making brick. In the gray gravel there are places where the upper strata have sunk down as if into a cavity, filling it with material belonging higher up. The uppermost strata of all are undisturbed in position as though the sinking had occurred, and the surface been washed level again, before the latter were laid down. One of the best examples was on the south side of the upper gravel pit, above Amiens, where the perpendicular bank showed a fine section of the phenomena. At this place the strata of gray gravel were missing for a horizontal distance of about four feet, the space being filled with confused materials from the dark colored strata above. The sides of the interrupted space were nearly perpendicular, and I at first supposed that an ancient pit or well had been dug there, and filled again from above. A closer inspection, however, showed that the supposed pit had never been open to the external air. Tracing upward from the bottom until I came to the thin stratum of dark gravelly clay, I found the following singular appearances. The dark stratum had evidently once been continuous across the space, and had never been removed by any excavation. It had, however, from some cause settled until it broke in the center, the ruptured ends flexing downward until they hung almost perpendicularly on each side of the space, while the material from above had pressed down and filled the interval. It was evidently not an artificial excavation, as it would have been impossible to dig in such a way as to leave the clay stratum in that singular position, and equally impossible for water to deposit it originally in such a form. The conclusion is inevitable, that a mass of some material was imbedded there which occupied the space until the gravel covered it and the clay stratum was laid over it. This mass then in some way disappeared, allowing the clay stratum to settle and break, and the upper gravel to press in and occupy the vacancy. What was this temporary mass? All the circumstances indicate that it was a block of ice, or at least of frozen gravel and ice combined, with enough of

the latter to leave a vacancy when it melted. An examination of the gray gravel adjacent confirms the idea of powerful ice action. It consists of chalk flints, mixed with broken chalk of every size, from a fine powder up to fragments as large as a man's head. Many of the fragments, though soft enough to write with upon a blackboard, have preserved with absolute perfection the sharp angles and edges which they had at the time they were broken from the cretaceous strata. It does not seem possible that they could have been rolled a hundred feet in the bed of a stream without losing this sharpness. It follows that much of the material of those beds was either dropped from floating ice, or else deposited by the mechanical action of ice fields floating down the ancient river, which crushed the edges of chalk strata abutting on the valley, and pushed the debris along to be left wherever the irregularities of the channel permitted. The agency of ice is further evinced by the occasional presence of large boulders of sandstone in the gravel, some of which weigh a ton. These must have been transported from far up the stream, as the rocks in the vicinity are exclusively chalk. The crushing action of ice against the edges of the valley is rendered more probable by the fact that the angular mixed gravel is found only in the border beds, while the strata nearer the center of the valley are washed comparatively clean of chalk.

The envelopment of ice and frozen gravel in sedimentary beds is by no means a phenomenon peculiar to the valley of the Somme. In a former article, I described the angular masses of loose clean gravel which occur in the drift clay beneath the bed of Lake Michigan. These masses occur in such shapes and positions as to show that they were deposited in a frozen state, and that they must have been completely covered with clay before they had time to melt, otherwise the loose gravel of which they are composed could never have maintained their peculiar forms and positions. These masses have furnished an extremely valuable means of estimating the rapidity of deposition of the drift clay of that locality.

If the phenomena in the valley of the Somme were produced in the way I have supposed, it follows that portions at least of the gravels there were deposited with extreme rapidity, insomuch as blocks of frozen material four feet in diameter were completely covered and had horizontal strata laid above them before they had time to melt.

The evidence of powerful force exhibited in the gravel beds of Abbeville and Amiens renders untenable Sir John Lubbock's theory that the valley was excavated by the imperceptible action of the river at about its present annual amount of water.

The valley of the Somme is over a mile and a half at the top, while the present river does not appear to exceed fifty feet in breadth. It is safe to say that the present stream spread over the whole valley, would not be half an inch deep, and making all probable allowance for spring floods, it is wholly inadequate to the production of gravel beds containing pebbles larger than a man's head, and boulders weighing a ton. This valley presents none of the characteristics of those which are widened by the irregular meandering of a shifting narrow stream, now eroding this bank and now that. It is broad, level-floored, and parallel-banked. The stream that excavated it filled it from bluff to bluff. Now the marginal gravel banks are often fifteen or twenty feet thick, and have horizontal strata. It follows that when the upper strata were laid down, the stream during its floods was a mile and a half wide and not less than twenty feet deep. It is proper to add, that farther up the stream the valley presents different characteristics and does not appear to have been formed altogether by erosion. Above Amiens it often widens out into vast irregular basins without definite boundaries. These appear to be the natural undulations of the surface already alluded to, and are connected to each other by valleys of erosion. If the land rose gradually from the ocean these wide inland basins would temporarily have the character of lagoons, and the flux and reflux of the tides would cut channels of communication in the soft chalk extending from the upper to the lower, thus sketching out the lines of the subsequent river beds. It is probable, therefore, that the outline of this river system was formed at the time of the elevation of the chalk from the sea, and that the valleys have been simply modified and not created by fresh-water erosion.

The conclusions to be drawn would seem to be these:

1. The ancient river, and consequently the ancient annual rainfall, was for a time immensely greater than at present.

2. The rapidity of the gravel deposit was, at least in places, very great, and the time required for it proportionately short.

Overlying the gravel of the floor of the valley is a bed of peat about twenty-six feet in thickness. Mr. Boucher de Perthes has with praiseworthy care sought for means to determine the age of this bed; but, as he was probably unacquainted with the phenomena of forest peats in process of actual formation, he has very excusably overlooked some of the most important data. Such an error is not to be blamed in Europe; because, where few trees are allowed to grow, and none to decay, the study of such phenomena is impossible. As the result of his observations, he concludes that the average increase of the peat has not exceeded four or five centimeters (1½ or 2 inches) in a

century. He states that there has been no perceptible accretion to it from time immemorial, so far as the observation of the modern inhabitants can determine. He believes that had the growth been more rapid than his estimate, the fact could not have escaped notice. Now the thickness of the peat being about 26 feet, an increase of 1½ to 2 inches a century would imply an age of from 15,600 to 20,800 years for the whole bed, or for the entire period since the termination of the gravel deposits. These figures must, however, be greatly modified before they can be accepted by one acquainted with actual forest life. Boucher de Perthes elsewhere states that he has found deep in the peats of the Somme numerous trunks of trees standing erect where they grew, generally birches, or alders. These trunks were sometimes a meter (39·14 inches) in height, but generally less. Now as stumps of trees do not stand long uncovered in the damp air of a swamp without decay, it follows that all which are found standing erect in the peat, must have been covered to their present summits with the increase, before they had time to rot away. Applying Boucher de Perthes's estimate of 1½ to 2 inches growth of peat in a century, it would follow that a stump one meter in height must have stood uncovered without decay from 1,950 to 2,600 years before the accretion of peat overtopped it and secured its preservation. The absurdity of the idea is obvious at a glance. One hundred years is a long duration to allow even an oak in such circumstances, and every trace of almost any other deciduous tree of the latitude would disappear in less than fifty years. Birch stumps are especially perishable. If, however, we allow a duration of a century to the tallest stumps, it is obvious that there are places in the valley of the Somme where the peat has increased three feet or more in a hundred years. This conclusion is confirmed by the existence of numerous prostrate trunks. Some oak logs have been found in the peat four feet in diameter, and so sound that they were manufactured into furniture: unless these trunks fell into a pond of water and were thus preserved, they must have been covered by the accretion of the peat before they had time to decay, say within a hundred years after they fell. The supposition that these trunks may have buried themselves by the force of their fall is not admissible, because no oak will bury itself by its fall in a soil solid enough for it to grow upon when alive.

The remark of Boucher de Perthes, that the growth of the peat is so slow as to be wholly imperceptible to the modern inhabitants, is doubtless true and very easily explained. The beds of the Somme belong to the class of forest peats, and not to that of the moss growths. Forest peats, as may be seen in

thousands of localities in the United States, are formed as follows: The annual crop of fruits, twigs, leaves, and windfall trunks, furnished by the trees and shrubbery of a dense swamp, amounts to an immense mass of vegetable matter. These added to a thick undergrowth of grass, herbs, and moss, are all pressed against the ground by the winter snows. In the spring they are flooded and protected from decay. In the summer they are partly protected from oxygenation by the extreme wetness of the soil into which they have been pressed. Hence they are only slightly rotted when they are finally covered up by the fall of the next autumn's crop. To one who studies the actual quantity of this material, a growth of two or three feet in a hundred years is by no means incredible. Thus the increase of the peat depends upon the presence of the forest; but the valley of the Somme has lost its forests centuries ago. It is wholly reduced to cultivation and pasturage. Hardly an ounce of grass or a stick of wood ever rots upon it, but every particle of vegetable matter is removed for the use of the inhabitants. About Amiens, the ground is all drained, and used for market gardens, and in other parts it is all sown with crops, mown for hay, or grazed for pasture. The peat growth, therefore, is arrested for want of material, and no further increase will be observed though a million years should elapse. Hence all calculations of age, based on the present want of progress, are necessarily erroneous.

In the excavations of the peat, Boucher de Perthes distinguishes near the surface relics of the middle ages: below that to the depth of about six feet Roman and Gallo-Roman remains, and underneath that, pure Gallic and other earlier traces. I have no means of knowing how long ago the forests of the Somme disappeared, but I presume those parts occupied by Roman garrisons would be among the larger settlements, and be earliest cleared of timber. Probably the places where Roman remains are found may have been destitute of timber, and produced little or no peat for the past six or seven hundred years. If so, the deposit of six feet of peat over the earliest Roman remains was accomplished in about 1200 years, or at the rate of about six inches in a century. This is much less than the rate required to preserve a stump three feet in height, but the latter was plainly a maximum. Most of the trunks were shorter, and in many places they seem to have disappeared altogether. While it is evident that the accretion in some times and places has been as high as three feet in a century, the average rate must have been lower, and very probably did not exceed six inches per century indicated by the depth of the Roman remains. If this be taken as a probable standard, the age of the whole bed, whose thickness is about 26 feet, would be not far from

5200 years down to the cessation of peat growth, or, adding the six or seven hundred years supposed to have elapsed since the clearing of the ground, the present age of the bed would be about 5800 years. It is impossible to pretend to minute accuracy in such calculations, but the above results are at least based on tangible data, and serve to show that in the eyes of practical woodsmen, the enormous European estimates of time greatly need pruning.

During my sojourn on the Continent, I twice examined the gravel cones of the Tinière, near Villeneuve, on Lake Geneva, which have been made familiar to geologists by the investigation of Morlot. Unfortunately I have not at hand his original account, and am obliged to take his statements second hand, as quoted by Sir John Lubbock and others.

At the eastern extremity of Lake Geneva, situated upon the level bottom of the valley of the Rhone, is the little city of Villeneuve. From the mountains which rise abruptly over the very edge of the city, descends the torrent of the Tinière, which is dry part of the year, but at other times becomes a stream some fifteen feet in breadth. This stream brings down annually a certain quantity of torrent gravel which has been deposited in the form of a cone, or rather of a half cone upon the level plateau on the border of the lake. The apex of the cone rests against the side of the mountain, and the base extends in a semicircle around the mouth of the gorge from which the torrent descends. A railroad has been cut through the cone, thus exposing a section of the mass nearly to its base. The cone was found to be stratified in a very regular manner, not horizontally, but in curves exactly parallel to its surface. About four feet below the top was a stratum of black soil which contained Roman relics. At the depth of ten feet, implements of bronze were discovered in a similar stratum, and at the depth of nineteen feet another black layer contained only articles belonging to the age of stone. Morlot very properly judged this a favorable opportunity to estimate the age of the whole cone, and by inference the duration of the present geological epoch, and of the human race. His calculation is as follows: The entire depth of the cone at the center of the section is thirty-two feet and six inches, while the Roman remains are only found to the depth of four feet. Some two or three hundred years ago the increase of the cone was stopped by confining the course of the torrent between stone walls, which prevented any further spreading of gravel over the surface, and caused it to be swept onward into the lake. Allowing 300 years as the probable time since the walls were built, there still remain about 1300 to 1500 years from the beginning of the Roman domination to the

cessation of the deposit, during which the cone was increased by the annual spread of gravel over its surface. It appears, therefore, that about four feet of gravel (1·14 meters) accreted in from 1,300 to 1,500 years, or not far from three and three-tenths to four inches in a century. Taking this ratio as a datum, Morlot, if I understand him correctly, applies it to the whole 32 feet 6 inches of depth, and decides accordingly, that the cone has an age of from 7,400 to 11,000 years. There are, however, two cones at Villeneuve. The one thus measured is the newer and smaller, and rests against the edge of a much larger one, which was formed before it, when the waters of the lake stood some fifty feet higher than at present. Now the upper cone is about twelve times as large as the lower, and, as Morlot infers, required twelve times as long for its accumulation, or from 88,000 to 132,000 years. The combined age of both cones, therefore, would by this calculation be between 96,000 and 143,000 years. It is with great hesitation that I question the conclusions of an European savant, made respecting his own country; but having twice examined these cones with great care, and followed the torrent a mile into the mountains to study its appearance and action, I cannot avoid the conclusion that there is a very singular mathematical error in estimating the age of the cones, and an omission of several important geological facts which vitiate the whole calculation. The nature of the mathematical error will be made obvious by a few facts. The gravel cones of Switzerland are very numerous, and the principle of their formation easily understood. On the supposition that the torrent brings down about the same amount of gravel each year, it will readily be seen that the first year's deposit will lie upon the plateau in a conical heap of no great breadth, but of considerable height. The second year's gravel however, will be spread over the entire surface of the first, and extending wider, it must be much thinner. The third year's accretion will be broader and thinner still, and so on to the last. It follows that the superficial annual layers are always the thinnest, because the broadest. Now, if Morlot is correctly quoted, he first derives his scale of from three and three-tenths to four inches increase per century from the superficial layers where they are thinnest, and then applies it without modification to the interior where the annual accretions were much thicker. His unit of measure is therefore too small, and exaggerates the total age. It is perfectly plain that the true method is to take the cubic contents of the strata whose age is known, and compare the amount with the cubic contents of the whole cone; or, in plain language, if the annual rainfall and gravel wash has been uniform, then as the quantity of gravel in the

layers deposited since the Roman conquest, is to the quantity in the whole cone, so is the time required for the deposit of these layers, to the time required for the formation of the whole cone.

The revised data and calculation would be as follows. The lower cone is really a half cone, the apex resting against the declivity of the upper one, and the base being a semicircle. The dimensions are nearly as follows:

Height of apex, 38 feet.

Radius of the base, 900 feet.

Cubic feet in the strata deposited since the Roman conquest, 5,283,205.

Time of deposition of the same, 1,300 to 1,500 years.

Cubic feet in the whole lower cone, 16,116,408.

Time of deposit of the same, 3,965 to 4,576 years.

Adding the 300 years, which have elapsed since the deposit ceased, the present age of the lower cone would be from 4,265 to 4,876 years.

It will be noticed that this is not far from the age which we estimated for the peat beds of the Somme. It would seem, therefore, that the period of higher water, which preceded both, was simultaneous at the two places.

The calculations respecting the upper and larger cone are vitiated not only by the overestimate of the age of the lower, on which that of the upper depends, but still more by leaving out of view several important geological facts.

The amount of gravel transported by a torrent, depends on the amount of water in it. Or, in other words, upon the annual rainfall. It is important to inquire, therefore, into this point. The fact that at the same period the river Somme had an immensely greater volume of water than now, and the water of Lake Geneva stood fifty feet above its present level, gives a probability that the torrent of the Tinière would be found affected by the same causes as the lake and the river. The examination of the upper cone itself settles this question, for it carries its history in the channels on its surface. The gravel cones of the Alps are marked with channels of a size proportioned to the quantity of water which flows in them, because the stones and gravel are left on each side of the water course up to high water mark, showing the exact size of the stream. As the gravel accumulates it gradually raises the bed of the torrent above the rest of the cone, until the water breaks through now on this side and now on that, and distributes its gravel over the lower portions. Hence the comparison of the torrent channels of the two cones, will show the size of the stream which formed them at the two periods. Now the bed of the

Tinière in the lower cone is only about 10 feet deep and 20 feet wide, while in the upper cone the old torrent bed is over 15 feet deep and 300 feet wide, making quite a valley, which in modern times is never one-twentieth part full of water, and is covered with vineyards. Other parts of the surface show traces of the torrent's magnitude when on its lateral wanderings. All the marks of water action in the upper cone are on a gigantic scale, compared with those of the lower. The same conclusion is supported by the appearance of the gorge from which the stream issues. For nearly a mile into the mountains, one may see distinct terraces of such size and position as to show that the stream has at some period been immensely larger than at present. It follows, therefore, that the rate of deposition during the formation of the upper cone was very much greater than in the lower, and the time occupied proportionately short, but we have no means of applying an exact measure for its determination.

It is possible, however, that this violent action may not have existed during the whole formation of the cone, and if so, the center may be older than the surface indicates. I believe the Alps at this point are considered to have been elevated in the earlier half of the Tertiary period, and I observed fragmentary terraces of glacial drift within the jaws of the gorge in such a position as to show that at that epoch the valley was already in existence, and that it had nearly its present size. It must be true that as soon as the mountains were elevated, the torrent commenced to flow, and to form a cone. Now if the tertiary cone was not removed by the changes of the drift period, it must still exist beneath the surface, and constitute part of the whole mass. In that case the age of the cones is not a measure of the duration of the present epoch, nor of man's antiquity, but of the present and of all the past periods as far back as the Miocene or Eocene. However this may be, the following conclusions seem highly probable:

1. The tranquil period of the lower cone was synchronous with the period of peat formation in the valley of the Somme, and their age would appear to be somewhere from 4,000 to 6,000 years.

2. The violent water action of the upper cone was cotemporaneous with the corresponding activity of the Somme in the gravel period.

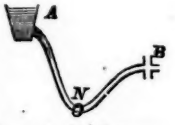
3. During this period, the water-flow was enormous, the gravel accretion extremely rapid, and the time occupied by the deposits correspondingly short.

ART. XXI.—*To ascertain the loss sustained in the delivery of water from a conduit pipe, when it is tapped by a branch at any portion of its length;* by THOS. GUERIN, Civil Engineer.

IN water works for the supply of a city it happens sometimes that it is necessary to convey the water in a conduit pipe from a reservoir situated at a long distance; and before the conduit reaches the point of distribution it is tapped by a branch so as to carry off a portion of the water for other special purposes, thereby interfering seriously with the proper supply for the city.

On examining the action of the branch it is easy to perceive that it causes an increase in the flow from the reservoir; while at the same time, it causes a diminution in the delivery at the point of distribution in the city, and moreover, the quantity carried off by the branch must be composed of such increase and diminution.

Thus, let A be a vertical section of a reservoir supplied from a river, or some other great source, and let ANB be a conduit pipe leading water to the point of distribution at B. It is manifest that if the pipe were cut at the point N, the delivery there would be greater than it had been at B before the pipe was cut, for the reason that a discharge through a pipe varies very nearly as the square root of the length inversely; it follows then that the discharge from the reservoir would be increased by cutting the pipe at the point N.



Again, if instead of cutting the pipe at N, an orifice were made in its side, such that the area of the orifice would be equal to the sectional area of the pipe, it is evident that almost as much water would flow through this orifice, as if the pipe were actually cut across at N (the only difference being the amount arising from a change in the direction of the fluid); while none can arrive at the point B, which is supposed to be a considerable distance removed from N. According as the orifice at N diminishes, so will the discharge from A diminish, and so will the water ascend toward B, until it commences to flow through at that point; and while the orifice continues to diminish, the flow will increase until it becomes a maximum, when N is closed; at that time the discharge from A is a minimum.

Again, if the orifice at N be made small at first, the delivery at B will be a little diminished, and while the orifice at N increases, this delivery will continue to decrease until finally it

becomes nothing; while the discharge from A will increase until it becomes a maximum as soon as the orifice at N is equal to the section of the pipe.

It follows then, that when a branch is inserted in a conduit, the effect will be to increase the discharge from the reservoir, but at the same time to diminish the quantity delivered at the point of distribution; and the sum of such increase and diminution will be the exact quantity carried off by the branch.

To be able to ascertain this increase and diminution separately, as well also as the exact quantity delivered at the point B, is manifestly of the greatest importance in laying a system of water pipes; and although writers on hydraulics have investigated formulæ for ascertaining the quantity passing in a branch, yet as far as I can ascertain, its component parts and also the exact amount delivered at the point of distribution do not appear to have been hitherto discussed. Toward a solution of this question, I submit the following investigation.

On reference to *D'Aubuisson's Hydraulics*, translated by Bennett, we find in chapter 3, No. 186,

$$H - \cdot 0251817\frac{Q^2}{D^4} = \cdot 0006769\frac{L}{D^5}(Q^2 + \cdot 141724QD^2).$$

This is a general formula employed for the solution of questions relating to the motion of water in conduit pipes; Q denotes the quantity flowing per second through the pipe; L, its length; D, its diameter, and H, the head on the orifice of efflux; the denominations of all these dimensions being feet.

The second member of the equation is the value of the resistance from friction occasioned by the sides of the pipe.

From this equation we obtain general values for H and Q as follows:

$$H = \cdot 0251817\frac{Q^2}{D^4} + \cdot 0006769\frac{L}{D^5}(Q^2 + \cdot 141724QD^2)$$

$$Q = -\frac{\cdot 070862LD^2}{L + 37 \cdot 20D} + \sqrt{\frac{1477 \cdot 30HD^5}{L + 37 \cdot 20D} + \left(\frac{\cdot 070862LD^2}{L + 37 \cdot 20D}\right)^2}.$$

Let us assume Q = the quantity delivered at B,
q = quantity carried off by branch,
L = length, NB,
l = length, AN,
H = height of reservoir above B,
D = diameter of pipe,
L = entire length of pipe, ANB.

On reference to the general value of H, it will appear that the head which forces the amount q to the point N, will be

$$\cdot 0251817\frac{q^2}{D^4}+\cdot 0006769\frac{l}{D^5}(q^2+\cdot 141724\,qD^2).$$

If H be diminished by this quantity and also by the friction due to the passing of Q through the length l, the remainder must be equal to the head which forces the quantity Q through the length, NB, viz:

$$H-\cdot 0251817\frac{q^2}{D^4}-\cdot 0006769\frac{l}{D^5}(q^2+141724qD^2)$$
$$-\cdot 0006769\frac{l}{D^5}(Q^2+\cdot 141724QD^2).$$

Let this quantity be substituted for H in the general value for Q as given above, and there will result

$$Q=-\frac{\cdot 070862LD^2}{L+37\cdot 20D}+\sqrt{\frac{1477\cdot 30D^5}{L+37\cdot 20D}\left(H-\cdot 0251817\frac{q^2}{D^4}-\cdot 0006769\frac{l}{D^5}(q^2+\cdot 141724qD^2)\right.}$$
$$\left.\sqrt{-\cdot 0006769\frac{l}{D^5}(Q^2+\cdot 141724QD^2)\right)+\left(\frac{\cdot 070862D^2L}{L+37\cdot 20D}\right)^2}.$$

Resolving this quadratic equation, the value of Q is found to be as follows:

$$Q=\frac{\cdot 070862D^2\mathbf{L}}{\mathbf{L}+37\cdot 20D}+\sqrt{\frac{1477\cdot 30}{\mathbf{L}+37\cdot 20D}(HD^5-\cdot 0251817q^2D)-\frac{l}{\mathbf{L}+37\cdot 20D}(q^2+\cdot 141724qD^2)}$$
$$+\left(\frac{\cdot 070862D^2\mathbf{L}}{\mathbf{L}+37\cdot 20D}\right)^2.$$

Such is the value for Q in general; but when the velocity exceeds two feet per second, and that of **L** is great compared to 37 times D, in that case it will be seen by D'Aubuisson, No. 189, that $\mathbf{Q}=37\cdot 548\sqrt{\frac{HD^5}{\mathbf{L}}}$, and hence $\mathbf{H}=\cdot 0007089\frac{\mathbf{LQ}^2}{D^5}$. From this general value for **H**, it appears that $\cdot 0007089\frac{lq^2}{D^5}$ is the head required to force the quantity q to the point N; and as it appears from D'Aubuisson, No. 187, that $\cdot 0007089\frac{lQ^2}{D^5}$ is the friction due to the passing of Q through the length l; it follows that $H-\cdot 0007089\frac{lq^2}{D^5}-\cdot 0007089\frac{lQ^2}{D^5}$ denotes the head which would be competent to force Q through the length NB. Let this quantity be substituted for **H** in the value for **Q**, and there will result $Q=37\cdot 548\sqrt{\frac{HD^5-\cdot 0007089\,lq^2}{\mathbf{L}}}$. This is a convenient formula for practice and it gives a correct result when the velocity exceeds two feet per second, and when the length is great

compared to 37·20 D; two circumstances which almost invariably occur.

The formula for $\mathfrak{Q}$ gives the delivery at the point of distribution before the branch is inserted; we shall therefore have $\mathfrak{Q}-Q=$ the loss sustained by the insertion of the branch; and $q+Q-\mathfrak{Q}=$ the increase of discharge from the reservoir arising from the same cause.

Department of Public Works, Ottawa, Nov. 14th, 1867.

ART. XXII.—*Notes upon some of the Mineralogical Curiosities of the Paris Exposition of* 1867; by WILLIAM P. BLAKE.

THE display of minerals in the department of Mining and Metallurgy, Class 40, of the exhibition was very extensive, and although composed chiefly of ores and metallurgical specimens, there were many objects of particular interest mineralogically, either for their rarity, their great size or beauty, or as coming from new localities. There was also a large display of precious stones, cut and polished, and of ornamental stones, both wrought and unwrought, such as columns, slabs, and vases of polished granite, porphyry, marble, and serpentine, or smaller objects of art in jasper, onyx, malachite, and lapis lazuli.

In these last mentioned objects the Russian collection was particularly rich. One of the most conspicuous was an elliptical vase about six feet high, sculptured at the Imperial establishment of Ekaterinburg out of a compact gray jasper from Kalkhansk. The shaft supporting the basin was ornamented by an entwined vine-branch with leaves and fruit exquisitely chiseled in high relief and polished. Two magnificent candelabra with pedestals of rhodonite were shown from the same establishment. These pedestals were about ten feet high, and nearly two feet broad at the base, but were probably formed of several pieces united. The rhodonite appears to occur in large homogeneous masses, and to be extensively employed for ornamental purposes. It takes a high polish, and has a pleasing rose-red color, slightly mottled with black.

Ivan Stebakoff, of Ekaterinburg, exhibited a great variety of beautiful paper-weights, made of polished slabs of jasper, malachite, and lapis lazuli, surmounted by groups of flowers or fruits in their natural colors, cut out of various highly colored ornamental and precious stones. The government establishment at Tiflis, Caucasus, sent numerous ornamental objects, some fashioned out of a kind of marble-onyx similar to the beautiful stalagmitic marbles of Algeria and Mexico, and others out of a peculiar chatoyant obsidian.

The wealth of the Russian empire in precious stones was well illustrated by a magnificent series of cut gems of the country from the Imperial cabinet. This contained amethysts, sapphires, emeralds, aquamarines, beryls, phenacites, white topazes, tourmalines, sphenes, and chrysoberyls, all of them of unusual size and brilliancy. Some of the phenacites which had a fine brown color when first shown, became bleached and colorless after exposure to the light.

The splendid displays of jewelry by the leading British and Continental jewellers contained a great variety of precious stones remarkable for their beauty and value. Hancock & Co., of London, exhibited a sapphire of fine color, one and a half inches long and three-quarters of an inch broad. One weighing 400 carats was shown by Mr. Mellerio.

Diamond.—In addition to the great numbers of large and well matched diamonds mounted in various ways for personal adornment, some of the exhibitors showed the gem in its various colors. Thus the following different colors were shown in a suite of nine beautiful diamonds in the case of Hancock & Co.: dark green, chrysolite-green, topaz-yellow, hyacinth-red, pink, cinnamon-brown, bluish-black with the luster of steel, and black with a lavender shade. A peculiar steel-like and graphite-like luster of the gem was well shown in a tiara and necklace formed of about forty stones, ranging from one-quarter of an inch to five-eighths of an inch in diameter, but not, however, wholly free from flaws.

In connection with the very interesting exhibition by Mr. Coster of Amsterdam, of the art of cutting and polishing diamonds, there was a remarkably fine display of the rough stones from all the principal diamond-producing localities. All the varieties of crystalline form and of color were shown, together with the minerals and rolled pebbles usually found associated with the diamonds in the deposits. In a suite of highly colored diamonds in this collection, there were two remarkable stones. One of 29 carats weight (about 92 grains), a pear-shaped brilliant, about three-quarters of an inch long, has the property of acquiring a rose-pink color, on being strongly heated. This color is retained in the dark after cooling, but if the gem is exposed to the light, the color soon vanishes. The experiment has been repeatedly performed with like results. Another stone with a beautiful bluish-black color by reflected light, appeared perfectly opaque when held between the eye and the light, owing, probably, to the total reflection of the light from the facets of the stone.

The collection contained masses of the crude black variety, known as "carbon," valuable for its powder. Irregularly shaped fragments of this are found with the diamonds of Bahia and

are sometimes two inches in diameter. Another variety called "boart," is translucent, and appears to be a confused crystallization; it accompanies the diamonds of Rio, and is also only valuable for its powder.

The *cascalho*, a conglomerate, containing diamonds, evidently corresponds in its origin to the "cement" of gold miners, and consists merely of the various water-worn and heavy pebbles accompanying the diamonds in the beds of streams, cemented together on the bed-rock by ferruginous, calcareous, or siliceous infiltrations.

The yearly importation of diamonds from 1862 to 1866 has averaged 176,000 carats, nearly 1,000 lbs. troy. Over 400 persons are constantly employed in cutting diamonds at Mr. Coster's establishment.

Opal.—Some remarkably large and fine opals were shown by Emil Goldschmidt, of Lower Austria. The largest one, oval in form, and about two and a half inches long and an inch wide, was valued at 75,000 francs. Another stone with a triangular outline, about one and a half inches on a side, was valued at 50,000 francs, and an ellipsoidal specimen, one and a half inches long and three-quarters of an inch in diameter, at 30,000 francs. Some large masses of the rock inclosing opals were also shown. It is evident from these specimens that the opal is sometimes formed in horizontal layers at the bottom of cavities, nearly as agates and the onyx are formed.

Turquois.—A fine series of turquois specimens, in the rough and cut, was shown by Petiteau, of Paris. Several of the cut stones were as large as pigeons' eggs and of good color.

Beryl.—One of the ornaments of a massive piece of goldwork consisted of a bust of the Emperor, about one-third life size, cut out of a single aquamarine of good color.

Rock Crystal.—Many very large and transparent crystals of quartz were sent from Japan, where this mineral occurs in abundance, and is wrought into many ornamental objects. An optical examination of some of these crystals by DesCloizeaux showed that, like quartz of most other localities, they were very irregularly compound in their interior structure, none giving a uniform tint in polarized light.

Platinum.—A variety of pepites, or nuggets, of platinum were shown in the Russian section from the mines of Prince Demidoff, in Tagilsk, government of Perm, Siberia. The largest mass weighed 13 pounds. Many of the smaller masses were highly magnetic, attracting iron filings like magnetite, and possessing strong polarity.

Native copper with native silver.—A mass of native copper weighing 1400 lbs., and resembling the copper of Lake Superior,

was sent from the Kirghise Steppes, Siberia. A small fragment of native silver, joined perfectly to the copper, as in the Lake Superior specimens, was observed in this specimen by Mr. Des-Cloizeaux.

Native silver.—The collection from the Kongsberg mines of Norway contained some extraordinary crystallizations of silver. Among these may be noted a cubic crystal three-quarters of an inch in diameter, with truncated angles; and a group of modified cubes, twenty or thirty in number, forming a mass about three inches in diameter. There were also some fine crystals of sulphuret of silver, and crystals of calcite penetrated with wire silver.

Proustite. — Some large groups of choice crystals of this species were in the collection from the mines of Chañarcillo, Chili, but the most extraordinary exhibition of this mineral species, in a massive form, was in the United States section, from the Poorman lode, Idaho Territory. The greater part of one of the masses weighing about 200 lbs. was composed of this mineral.

Torcornalite.—Among the silver ores sent from Chili, Mr. Domeyko notes a new species to which he has given this name; a double iodid of silver and of mercury, from the Alfinhallada mine, Tres Puntas.

Cryolite.—A mass of this mineral, three feet long and two feet thick, from Iviktout, Greenland, was exhibited by the Greenland Cryolite Mining Company. Nearly 20,000 tons of this mineral were shipped in 1866.

Allanite.—In large crystals from Avegeit, Greenland.

Iceland spar.—From Helgostad, Iceland. The end of a large crystal 2½ feet long and two feet in diameter at the base. One of the faces was studded with small implanted crystals of stilbite. The mass appears to be quite sound and clear, and, with other fine cleavage masses, to be suitable for optical purposes.

Malachite.—The Peak Downes Copper Co., of Queensland, exhibited a mass of malachite five feet three inches long and ten inches thick. A specimen from the Medno-Roudiansk mine, Siberia, the property of Paul Demidoff, weighed 2150 kilograms, about 4686 pounds, and was valued at 75,000 francs. Large masses of malachite were first found in 1840, since which upward of 1,260,000 lbs. have been taken out.

Graphite.—Mr. Alibert made a very interesting exhibition of graphite from Mount Batougol, near Irkoutsk, Siberia. It is remarkable for its uniform density, freedom from grit, for its blackness, and for a peculiar fibrous structure which shows upon the surface of the blocks like the grain of split wood. This

structure has already been noted by General Kokscharov in his Mineralogy of Russia. Most of the masses exhibited are in the form of prismatic columns from one to three feet long, and a few inches only in thickness, looking as if they had been split out. It may be sawn and cut into any desired shape and gives sharp and firm angles and points, and is apparently well adapted for pencils, for which it is largely used.

Nephrite.—The exhibitor of the Siberian graphite sent, also, a large polished block of nephrite of dark green color weighing 1,000 lbs.

Stassfurth Mine Products. — This remarkable deposit of potash salts is being explored with increasing success, and its nature and products were well shown by specimens of the different minerals in connection with a very carefully made model. A large arched recess was built out of large blocks of the rock salt, in which the numerous wavy sheets of anhydrite could be easily seen. Glass jars arranged about this recess contained samples of carnallite, boracite, tachhydrite, kieserite, polyhalite and kainite.

Borax.—A very interesting series of specimens from the Borax lake in California was shown in the collections from that state. Some of the crystals were nearly six inches long, and from two to three inches thick. Smaller crystals occur of all sizes, and they are all found in a thick blue clay at the bottom of the lake.

Sulphur.—Mr. J. Mottura of Turin, exhibited a fine collection of crystallized sulphur, celestine, strontianite, and gypsum from Sicily.

ART. XXIII.—*Notes on the Lignite Deposits of the West;** by F. V. HAYDEN, U. S. Geologist. Published by permission of the Commissioner of the U. S. General Land Office.

THE construction of the Pacific Rail Roads across the continent is bringing about the dawn of a new era in the progress of the west. Already has the U. P. R. R., from Omaha, struck the first range of the Rocky Mountains, more than 525 miles west of the Missouri river. The earth is now called upon more earnestly than ever before to yield up her treasures of gold, silver, copper, iron, and mineral fuel, and the existence of the last two minerals in the west, in workable quantities, is one of the most important practical questions of the day. It is my purpose in this article, merely to state briefly some observations

* Abstract from forthcoming Report of U. S. Geological Survey of Nebraska.

made last autumn in regard to the lignite deposits of Colorado and Dakota territories. The details will be given more fully in the final report of the Geological Survey of Nebraska, now in progress of preparation.

The discovery that large deposits of "stone coal," as it is often called by travelers, existed in various portions of the west is by no means a new one, at the present time. The lignite beds of the Upper Missouri were noticed by Lewis and Clark, 1803 and 1804, those of Laramie Plains by Fremont, 1842, and those of the Raton Mt. region by General Emory, as far back as 1848. But the intense interest with which they are regarded now, as a source of fuel to the vast stretch of fertile but almost treeless plains, has been created anew by the advancing westward wave brought about by the construction of those great national highways. The fact, also, that the coal deposits of Iowa and Missouri are restricted in area and the coal limited in quantity, and in most cases inferior in quality, and that west of these states it may be said that there is no true coal at all, renders any source of fuel in the far west, a matter of the greatest importance. In the valley of the Missouri river and the Yellowstone, there are numerous beds of Tertiary lignite, varying from a few inches to seven feet in thickness. These formations have been described many times, and until the northern Pacific R. R. is carried through that region, they will remain of little practical importance. But the U. P. R. R. is now in progress of construction through the lignite deposits of the Laramie Plains, and the U. P. R. W. E. D., and the branch from Denver to Cheyenne City will pass through those of Colorado, so that if the lignite beds and iron mines of this region are of such a character as to be of economical use, the time for their demand has already come.

My examination of the geology of the State of Nebraska, during the past season, failed to develop any workable beds of coal within the limits of that state. My attention was then directed to the great lignite deposits of the Laramie Plains. I found the lignite to be of excellent quality in beds from 5 to 11 feet thick, and I estimated the area occupied by this basin at 5,000 square miles. Its most eastern limit is about 10 miles east of Rock creek, a branch of the Medicine Bow river. Outcroppings have been seen all along Rock creek, Medicine Bow, on Rattlesnake Hills, on the North Platte, Muddy creek, all along Bitter creek, Hams' Fork, Echo Cañon, and all along Weber river, nearly to great Salt Lake, showing that one connected series of deposits covers this whole area. The lignite taken from the beds on Rock creek is from the outcroppings, yet it burns with a bright red flame, giving out a good degree of heat, leaving

scarcely any ash, and is quite as desirable fuel for domestic purposes as any wood. Indeed it might be called condensed dry wood. It is non-bituminous, exhibits just a trace of sulphuret of iron, which decomposing gives a rusty reddish appearance to the outcrops, and there are seams of jet one to twelve inches in thickness, which looks much like cannel coal and is thus termed by the miners. The Union Pacific Rail Road will pass directly through these great coal fields, and as most of the freight will go westward for many years, the cars on their return can be loaded with this lignite, thus to be distributed through Nebraska at a cost much less than that of wood at the present time. There are also indications of an abundance of iron ore in the vicinity of these deposits, and the U. P. R. R. Co. contemplate establishing rolling mills in the Laramie Plains at no distant period.

The next point visited was South Boulder creek, the Marshall mines, which are probably the most valuable in the west. I made a pretty careful examination of these mines, as they have been wrought for four or five years. An average of 50 tons is taken from this place daily and sold at Denver, at prices varying from 12 to 15 dollars per ton. The beds are at the foot of the mountains and dip to such an extent as to expose the whole series, 11 in number, varying from 5 to 13 feet in thickness, so that we have from 30 to 50 feet at least of solid lignite. This is the most favorable locality for studying the strata inclosing the lignite that I have ever met with in the west, and this is due to several causes, the principal of which is their proximity to the base of the mountains, by which they are elevated at a moderate angle. The following somewhat remarkable section is approximately correct at least.

45. Sandstone, gray and rather coarse grained.
44. Drab clay.
43. Lignite.
42. Drab clay.
41. Lignite.
40. Drab clay.
39. Sandstone.
38. Drab clay.
37. Lignite.
36. Drab clay.
35. Sandstone.
34. Drab clay, 10 to 12 feet.
33. Sandstone.
32. Drab clay.
31. Lignite.
30. Drab clay.
29. Sandstone,
28. Drab clay passing up into sand, 3 feet.
27. Lignite, 5 feet.
26. Drab clay, 5 feet.
25. Sandstone, 14 feet.
24. Drab clay, 3 feet.
23. Lignite, 7½ feet.
22. Drab clay, 5 feet.
21. Sandstone, 20 feet.
20. Drab clay, 3 feet.
19. Lignite, 7 feet.
18. Drab clay, 3 feet.
17. Sandstone, 40 feet.
16. Drab clay, 3 feet.
15. Lignite, 5 feet.
14. Drab clay.

13. Sandstone.
12. Drab clay.
11. Lignite.
10. Drab clay.
9. Sandstone.
8. Drab clay.
7. Lignite, 5 feet.
6. Drab clay.
5. Gray and yellowish gray sandstone.
4. Drab clay, 3 feet.
3. Lignite, 11 to 13 feet.
2. Drab clay, 4 feet.
1. Fine yellowish grit indurated.

Cretaceous beds, 1, 2, 3, 4, &c.

The thickness of the beds is given when it could be obtained with any degree of accuracy. It is barely possible that beds 6 to 13 feet inclusive have been broken down from the summit of the upheaval just beyond and thus displaced. The inclination of the strata from 1 to 16 inclusive is 8° east, and the cleavage of the beds of lignite is vertical and exactly parallel with the dip. From 13 to 29, inclination is 40°, and the remainder, 35°. Lignite beds 38 and 42 have not yet been tested and very little is known of them. They have been exposed in the search for iron ore. The summit of the hills above all these beds in the section is covered with a large thickness of superficial drift material, which undoubtedly conceals many other beds which properly belong to the section. Mines have been opened on Coal creek, 3 miles south of Marshall's mines, but they have been abandoned for the present. Another has been opened about 20 miles south of Cheyenne City, on Pole creek. The drift began with an outcropping of about 4 feet 8 inches in thickness, inclination 12° east. The lignite grows better in quality as it is wrought farther into the earth, and the bed, by following the dip 200 feet, is found to be 5 ft. 4 in. thick, and the lignite is sold readily at Cheyenne City for $25 per ton. The beds are so concealed by a superficial drift deposit, that it is difficult to obtain a clearly connected section of the rocks. A section across the inclined edges of the beds eastward from the mountains is as follows:

7. Drab clay passing up into areno-calcareous grits composed of an aggregation of oyster shells, *Ostrea subtrigonalis*, ----------
6. Lignite, ---------- 5 to 6 feet.
5. Drab clay, ---------- 4 to 6 "
4. Reddish rusty sandstone in thin laminæ, ---------- 20 "
3. Drab arenaceous clay, indurated, ---------- 25 "
2. Massive sandstone, ---------- 50 "
1. No. 5 Cretaceous apparently passing up into a yellowish sandstone, ----------

The summit of the hills near this bed of lignite is covered with loose oyster shells, and there must have been a thickness

of 4 feet or more, almost entirely composed of them. The species seems to be identical with the one found in a similar geological position in the lower lignite beds of the Upper Missouri near Fort Clark, and at the mouth of the Judith river, and doubtless was an inhabitant of the brackish waters which must have existed about the dawn of the Tertiary period in the west. No other shells were found in connection with these in Colorado, but on the Upper Missouri well known fresh-water types exist in close proximity, showing that if it proves anything, it rather affirms the Eocene age of these lower lignite beds. These lignite beds are exposed in many localities all along the eastern base of the mountains, and from the best information I can secure, I have estimated the area occupied by them north of the Arkansas river at 5,000 square miles. According to the explorations of Dr. John L. LeConte during the past season, which are of great interest, these same lignite formations extend far southward into New Mexico on both sides of the Rocky Mountains. Specimens of lignite brought from the Raton mountains by Dr. LeConte, resemble very closely in appearance and color the anthracites of Pennsylvania. It is probable that no true coal will ever be found west of longitude 96°, and it becomes therefore a most important question to ascertain the real value of these vast deposits of lignite for fuel and other economical purposes. Can these lignites be employed for generating steam and smelting ores? In regard to the lignites in the Laramie plains, I have as yet seen no analysis, but specimens are now in the hands of Dr. Torrey of New York, for that purpose; specimens from Marshall's mine on South Bolder creek were submitted to Dr. Torrey by the U. P. R. R. Co. for examination, with the following result:

Water in a state of combination, or its elements,	12·00
Volatile matter expelled at a red heat, forming inflammable gases and vapors,	26·00
Fixed carbon,	59·20
Ash of a reddish color, sometimes gray,	2·80
	100·00

A specimen from Coal creek, 3 miles south, yielded similar results.

Water in a state of combination, or probably its elements as in dry wood,	20·00
Volatile matter expelled at a red heat in the form of inflammable gases and vapors,	19·30
Fixed carbon,	58·70
Ash consisting chiefly of oxyd of iron, alumina, and a little silica,	2·00
	100·00

The percentage of carbon is shown to be in one case, 59·20, and in the other, 58·70, which shows at a glance the superiority of the western lignites over those found in any other portion of the world. Anthracite is regarded as so much superior a fuel, on account of the large per cent of carbon, and also the small amount of hydrogen and oxygen. The bituminous coals contain a large percentage of hydrogen and oxygen, but not enough water and ash to prevent them from being made useful, but the calorific power of lignite is very much diminished by the quantity of water contained in it, from the fact that so valuable a portion of the fuel must be used in converting that water into steam.

The day of my visit to the Marshall coal mines on South Boulder creek, 73 tons of lignite were taken out and sold at the rate of $4 a ton at the mine, and from $12 to $16 at Denver. This lignite is somewhat brittle but has nearly the hardness of ordinary anthracite, which it very much resembles at a distance.

In some portions there is a considerable quantity of amber. I spent two evenings at Mr. Marshall's house, burning this fuel in a furnace, and it seemed to me that it would prove to be superior to ordinary western bituminous coals and rank next to anthracite for domestic purposes. Being non-bituminous it will require a draft to burn well. It is as neat as anthracite, leaving no stain on the fingers. It produces no offensive gas or odor, and is thus superior in a sanitary point of view, and when brought into general use, it will be a great favorite for culinary purposes. It contains no destructive elements, leaves very little ash, no clinkers, and produces no more erosive effects on stoves, grates, or steam boilers, than dry wood. If exposed in the open air it is apt to crumble, but if protected it receives no special injury. Dr. Torrey thinks there is no reason why it should not be eminently useful for generating steam and for smelting ores.

Throughout the intercalated beds of clay at Boulder creek and vicinity are found masses of a kind of concretionary iron ore, varying in size from one ounce to several tons in weight. This iron ore is probably a *limonite* commonly known under the name of brown hematite or brown iron ore. It may perhaps be found in the state of carbonate of iron when sought for beyond the reach of the atmosphere. These nodules or concretionary masses, when broken, show regular concentric rings varying in color from yellow to brown, looking sometimes like rusty yellow agates. It is said to yield 70 per cent of metallic iron. The first smelting furnace ever erected in Colorado, was established here by Mr. Marshall, and he informed me that

for the production of one ton of pig iron, 3 tons of the ore, 200 pounds of limestone, and 130 to 150 bushels of charcoal are required. Over 500 tons of this ore have been taken from this locality and the area over which it seems to abound cannot be less than 50 square miles. Indications of large deposits of iron ore have been found in many other localities along the line of the Pacific Rail Roads, and if the mineral fuel which is found here in such great abundance can be made useful for smelting purposes, these lignite and iron ore beds will exert the same kind of influence over the progress of the great west that Pennsylvania exerts over all the contiguous states. When we reflect that we have from 10,000 to 20,000 square miles of mineral fuel in the center of a region where for a radius of 600 to 1,000 miles in every direction there is little or no fuel either on or beneath the surface, the future value of these deposits cannot be over-estimated.

The geological age of these western lignite deposits is undoubtedly Tertiary. Those on the Upper Missouri have been shown to be of that age both from vegetable and animal remains, and in the Laramie Plains I collected two species of plants, a *Populus* and a *Plantanus*, specifically identical with those found on the Upper Missouri. The simple fact that Cretaceous formations Nos. 1, 2, 3, 4 and 5, are well shown all along the foot of the mountains, and that No. 5 presents its usual lithological character with its peculiar fossils, within 15 miles of Marshall's mines, also that at the mine, 2, 3 and 4, are seen inclining at nearly the same angle and holding a lower position than the lignite beds, is sufficient evidence that the strata inclosing the lignite beds are newer than Cretaceous. A few obscure dicotyledonous leaves were found, which belong rather to Tertiary forms than Cretaceous.

The connection of the lignite deposits on the Upper Missouri has been traced uninterruptedly to the North Platte, about 80 miles above Fort Laramie.

They then pass beneath the White river Tertiary beds, but reappear again about 20 miles south of Pole creek, and continue far southward into New Mexico. Near Red Buttes, on the North Platte, it seems also probable that the same basin continues northward along the slope of the Rocky Mountains nearly or quite to the Arctic sea. Whether or not there are any indications of this formation over the eastern range in the British possessions, I have no means of ascertaining, but the Wind river chain, which forms the main divide of the Rocky mountain range, exhibits a great thickness of the lignite Tertiary beds on both eastern and western slopes, showing conclu-

sively by the fracture and inclination of the strata, that prior to the elevation of this range, they extended uninterruptedly in a horizontal position across the area now occupied by the Wind river chain. Passing the first range of mountains in the Laramie plains, we find that the Big Laramie river cuts through Cretaceous beds, Nos. 2 and 3, continuing our course westward to Little Laramie, a branch of the Big Laramie, and No. 3 becomes 50 to 150 feet in thickness filled with fossils, *Ostrea congesta*, and a species of *Inoceramus*. At Rock creek, about 40 miles west of Big Laramie river, the lignite beds overlap the Cretaceous, but in such a way as to show that the more inclined portions have been swept away by erosion, and that the red beds and Carboniferous limestones once existed without break and in a horizontal position across the Laramie range prior to its elevation.

I cannot discuss this matter in detail in this article, but the evidence is clear to me now, that all the lignite Tertiary beds of the west are but fragments of one great basin, interrupted here and there by the upheaval of mountain chains or concealed by the deposition of newer formations.

All the evidence that I can secure seems to indicate that there are no valuable beds of lignite west of the Mississippi, in formations older than the Tertiary.

Postscript.

After my article on the Lignites of the West was in type, Professor Lesquereux sent me the following very valuable notes, as the result of his preliminary examination of the fossil plants of the Lignite deposits. His conclusions seem to confirm my statements that all these formations are probably of Tertiary age.

Species from Rock creek, Laramie Plains.

1. *Populus attenuata* Al. Braun. The identity of these leaves with the European species is undoubted.
2. *Populus lævigata*, sp. nov., related to *P. balsamoides* Göpp., a species, which, like the former, is abundant in the Miocene of Europe.
3. *Populus subrotunda*, sp. nov. Type of neuration of *P. melanaria* Heer, and form of leaves of *P. mutabilis* Heer, both species also common in the Miocene of Europe.
4. *Quercus acrodon*, sp. nov., a fine oval leaf resembling a chestnut leaf, related to *Quercus prinoides* Wild, of our time.
5. *Quercus Haydeni*, sp. nov., lyrate leaf with lobes strongly dentate, without near relation to any species, either of the Tertiary or of our time.

6. *Platanus aceroides* Göpp., one of the most common species of the Miocene of Europe. It is closely related to, if not identical with *P. occidentalis L.*, of our time.

Marshall's mine (near Denver).

1. *Quercus chlorophylla* Ung. Three specimens of this species have been figured and described in my paper, "On species of fossil plants from the Tertiary of Mississippi" (Trans. Phil. Soc., vol. 13, pl. xvii, fig. 5, 6, 7). It is still uncertain if these leaves represent a *Quercus*, but all belong to the species described and figured by Heer under this name, and common in the whole thickness of the European Miocene.
2. *Quercus Lyelli* Heer, also figured in the above paper, pl. xvii, figs. 1, 2, 3. Though the specimen is somewhat obscure, the essential characters which distinguish the species are well discernible. It is abundant in the Bovey Tracy lignite formations of England, Lower Miocene.
3. *Cinnamomum affine*, sp. nov. This species is also found at Raton pass. The leaf from Raton pass is smaller and might belong to a different species, but except the size I do not find ground for separation: very near *C. Mississippiensis* Lesq., and also closely related to *C. Buchi* Heer, of the Lower Miocene of Europe.
4. *Cornus incompletus*, sp. nov. A part of a leaf apparently round at the top, general outline uncertain. It is figured merely for future reference. By its peculiar nervation this leaf appears in close relation to, if not identical with *Cornus rhamnifolius* Web. Pretty common in the lower Miocene of Europe.
5. There are, in the Marshall's shales, a few fragments of Maple leaves (*Acer*) specifically undeterminable, and also one winged seed of this genus. This seed has a narrow straight wing like that of *Acer trilobatum* Heer, but with smaller nutlet.
6. *Rhamnus salicifolius*, sp. nov., in soft sandstone; related to *R. marginatus* Lesq., and also to *R. Carolinianus* Walt., now living and abundant in southern swamps.
7. *Juglans rugosus*, sp. nov., very nearly related to *J. acuminata* Al. Braun, a species extensively distributed in the European Miocene.
8. *Echitonium Sophiæ* Web. The leaf has no visible nervation, but it is exactly like both the forms represented from European specimens. It is found in the whole Miocene of Europe, especially in the lower stage.
9. *Phyllites sulcatus*, sp. nov. The borders of the leaf are destroyed, but the nervation is quite peculiar. It is referable either to a *Rhodora* like *R. Canadensis* of our time, or represents merely the lower part of the winged petiole of the fruit of a Linden (*Tilia*).
10. *Lygodium compactum*, sp. nov. Though many species of Lygodiums are described from the Tertiary of Europe, none are related to ours. One lobe of a leaf only is presented, and the

general outline of the leaf is therefore unknown, but the nervation which is very close and more like that of a *Neuropteris*, is of a peculiar character.

Lignite beds near Golden City, Colorado.

1. *Magnolia tenuinervis*, sp. nov. Not possible to indicate the general form of the leaf of which a part only is presented. Its thin and sharp secondary nerves distinguish it from any other fossil species.
2. *Lathræa arguta*, sp. nov. May be a *Pecopteris*. No relation observed of any known species to this one.

Raton Pass. Specimens collected by Dr. LeConte.

1. *Berchemia parvifolia*, sp. nov. Related to *B. multinervis* of the European Miocene, but still more like our *Berchemia volubilis* which fills the southern swamps. The basilar part of the leaf is not seen and therefore a satisfactory determination is not possible.
2. *Abietites dubius*, sp. nov.

Most of the specimens from Raton pass have some remains of leaves or branches of a coniferous species which can be referred, perhaps, as well to the genus *Araucaria* as to *Sequoia* or *Abies*. As the leaves on the branchlets appear evidently placed around the stems and not on both sides of it, and as the scars left on the bark are of the same form as those of an *Abies*, I place these remains in this genus till they may be studied on better specimens. The leaves are pointed as in *Taxites dubius* Göpp., from the Tertiary of Europe; except this, these remains have no analogy with any other, published or figured.

3. *Echitonium Sophiæ* Web. A small fragment exactly like those of Marshall's coal bed and a specimen of *Cinnamomum affine* already mentioned from the Marshall's shales.

Upper end of Purgatory cañon, Dr. LeConte.

1. *Rhamnus obovatus*, sp. nov. All the specimens are from the same place, and all contain fragments of the same species, and none of any other. This species is peculiar by the form of the leaves; it has the character of a *Rhamnus* but the secondary nerves are closer and more numerous than in any other species of the genus, even more so than in a *Berchemia*. I do not know of any fossil plant comparable to this.

From this short report on your fossil plants examined till now, it is easy to draw some general conclusions.

From Rock creek we have only six species. Two are identical with species from the Miocene of Europe, and one of them, *Platanus aceroides*, is not distinguishable from our *P. occidentalis*. Two other species are closely allied to European Tertiary species. And of the two others, one is an American type related to *Quercus*

prinoides, still in our flora, the other a peculiar and lost type. The appearance of this florula is quite modern. This may be the result of geographical circumstances. Poplars and Buttonwoods live together in the bottoms of rivers, and therefore I may mistake in believing this Rock Creek formation more recent than that of Marshall's. In any case it is certainly Tertiary and has no plants of an older formation.

In Marshall's (coal beds) we find only ten species of fossil plants, two *Quercus* and one *Echitonium* apparently identical with Miocene species of Europe, one *Rhamnus* closely related to a living species of ours, and at the same time to a fossil species of the lignite of Mississippi, one *Cornus*, one *Juglans*, and one *Cinnamomum*, all related to Miocene species, and the last one also closely allied to a species of the Mississippi Tertiary; undeterminable leaves of Maple, seeds of the same genus, a *Lygodium* and an undeterminable *Phyllites* complete the list. These plants have, therefore, all of them, the character of Tertiary plants. The general aspect of the Marshall coal flora is that of the Mississippi lignite, which I consider as either lowest Miocene or Eocene. In this I am much pleased to find my views so well agreeing with yours.

The materials obtained from the strata of Golden City, Raton Pass and Purgatory cañon, are too scanty to permit considerations in regard to the geological positions of the strata which have furnished them. No *Abies* has yet been described from Tertiary strata, but with these broken remains of a conifer of uncertain genus, the shale of Raton Pass has a *Berchemia*, which is a Tertiary plant, and a leaf of *Echitonium*, and one of *Cinnamomum* identical with specimens found at Marshall's.

The remarks as given above, were sent to me by Mr. Lesquereux, in the form of a letter, and all the important facts contained in it are given, omitting unimportant details. I regard these results as extremely interesting, throwing much clear light on many obscure points in western geology. That the Marshall beds are lower Tertiary, may be inferred not only from the organic remains, but also from the entire conformity, so far as can be seen, of Cretaceous and lignite beds. It will be difficult also to draw any line of separation between Eocene and Miocene in the lignite formation in any part of the West.

ART. XXIV.—*Some Remarks on the recent Geological changes in China and Japan;* by ALBERT S. BICKMORE, M.A.

IN the old and accurate description of China, compiled by du Halde in about the year 1725, from the full diaries and journals kept by the Jesuits, who between 1708 and 1717 traveled over and mapped out all that vast empire, I find these noteworthy remarks on the changes in the physical geography of China, since her earliest history.

"In the abridgment of Chorography entitled Kwang-iu-Ki, we find that the city Chantsien (the capital of Corea in 1694), where Kipe (the king of Corea at that time) resided, is in the territory of Yungping fu, a city of the first order in the province of Pechili."

"Now, supposing this to be true, one may reasonably conclude that the ancient Chantsien and Corea were *contiguous*, and not separated by a gulf till many ages after. For it is not to be imagined that a prince would fix his residence out of his dominions, especially if divided from them by a wide sea. This conjecture will appear the more probable if we carry our speculations a little higher (i. e., to a more ancient period).

"When Yu (whose memory is justly honored by the Chinese with the title of great) undertook to drain the waters, which under the reigns of Shun and Yan had overflowed the flat country, he cut a passage for the river Hwang Ho (or Hoang Ho), i. e., the Yellow river, through a mountain on the southern boundaries of Shansi and Shensi, which provinces that river separates, and makes a cataract here not inferior to those of the Nile. Thence he conducted it through the province of Honan, and following its channels along the province of Pechili, he drained the lake Talu (i. e., Pehlu) into which the Hwang Ho formerly emptied itself. This lake overflowed all that country which now includes the districts of Shunti fu, and Chauchau and Sinchau in the same province. At last to break its rapidity, he divided it into nine channels, which some imagine were again united before it disembogued itself into the sea. But whether they were joined, or if it was only the main channel that ran into the sea at the foot of the mountain Ki-she-shan, which was then made a promontory, this is certain, that since Yu began that great work (about 3921 years ago), this river *has* strayed so far from its ancient course; for instead of discharging itself into the sea as it did formerly in lat. 40° it now falls into the river Hwang Ho, a little above Hwaingan, a city of the first rank in the province of Kiangngan about lat. 34°.

It is likewise observable, that the mountain Ki-she-shan, which formerly was united to the territory of Yungping fu, is now *five hundred* li (*about one hundred and sixty English miles*) distant in the sea from this city (and now, 1867, it probably forms one of the islands near the promontory north of the Miautau islands). So that the sea gaining on the land by degrees has at last overflowed all this tract of ground. It is indeed true that the Chinese history makes no mention of this extraordinary change of the course of the Hwang Ho, nor of this overflowing sea. But when alterations on the surface of the globe are brought about insensibly, and without alarming nature, they easily escape the observations of history, the difference that happens during the life of one man being not at all perceptible."

In October, 1866, after a long journey through the southern and central part of China, from Canton to the Tungting lake, and down the Yangtse to Haukow and Shanghai, I took passage for Tientsin through the kindness of my good German friends Trautmann & Co., on their steamer, the "Nanzing." My object in part was to note what evidences might appear of recent geological changes in the plain of Pechili, and to visit the coal mines near the capital of the empire. From Tientsin I proceeded directly to Peking, and thence to the mountains on the north, and along the flanks of this range to the Nankau pass, and thence to the coal mines, a day's journey southward. And in entire accordance with Father du Halde's statements and speculations given above, everywhere there appeared evidences of a recent elevation above the sea. A consideration of the charts of the gulf of Pechili and the Yellow sea, farther showed me that the true eastern border of this plain is not the present sea shore, but that this plain *continues out under* the gulf and under the Yellow sea, on the north to Corea, and on the south of Shantung even to the Japanese Islands, the Lew Chews and Formosa. In other words, this plain extends out eastward to the mountain range which stretches along the proper eastern edge of the Asiatic continent, the true Pacific basin beginning only eastward of the islands just named.

If the north of China were to be raised but one hundred and twenty feet, the whole gulf of Pechili would become *dry land*, and if it were elevated as much more, in place of the Yellow sea there would be a continuous plain from Peking to Corea, and *such* a change is now actually going on. At Chefoo, on the northern side of the promontory of Shantung, there is a long sand-pit extending out from the mainland to a high headland, and forming the western side of the harbor. On this spit are seen two old sea beaches, as perfect as the present one.

The highest is but a few feet above the sea, yet it shows what kind of a change the surrounding area has recently undergone, and this is farther strengthened by the testimony of all the Chinese, that "the harbor is slowly filling up."

It is seldom possible to obtain data that will give us the rate of such changes, and therefore the following statements kindly furnished me by the Rev. Mr. Metier at Tungchau, are the more valuable.

"I have learned with some degree of probability that in the Ming dynasty, some 250 years ago, the water from the sea came up the bed of what is now a small creek, past the city, and extending as much as three li (one mile) from the present beach. I also learned that at that time, the water in the basin of the "Water city," was at least *fifteen* Chinese feet deep where now it is not *three* feet deep. This difference of twelve Chinese ($14\frac{1}{6}$ English) feet is the *least* that will account for the water running up to where it is said to have done. At a later date, the water came up outside the water city to the bridge near a li (one-third of a mile) from its present beach. The decline has been so gradual, that there is now no way of telling exactly when the junks quit coming. It is said that there was originally no dry land in the water city, save the very least close to the wall. It was built by a man named Chi, as a depot for the government junks, and was not intended to include any place to build upon, but to be a safe depot, where pirates could not come."

This elevation of at least $14\frac{1}{5}$ English feet in 250 years, gives a mean rate of nearly five feet in a century, and to realize the whole elevation of the bed of the gulf of Pechili, we must add to this rising, an estimate of the quantity of sediment probably brought down by the Yellow river, the Peiho and minor streams.

If this area had subsided 14 feet during the last two centuries and a half, instead of rising 14 feet, of course it would have been 28 feet below its present level, and probably *one-third of the low, thickly populated parts of China would then be beneath the sea.*

About Peking the plain of Pechili is composed of stratified clays, that form in the dry weather a fine, light dust, from two to five inches deep. When the heavy winds of autumn and winter set in with violence, this dust rises in one continuous mass to the very clouds, and forms the dust storms for which Peking is so justly famous.

As I journeyed over the plain, I noticed in many places considerable quantities of clay stones closely resembling branching corals, for which, indeed, they have been mistaken. But instead of being of marine origin, they are formed by the water trick-

ling down along the grass roots, which in this way receive a series of concentric layers from the surrounding clay. And as the water follows the various branches of the roots, these clay stones necessarily take a branching form. Frequently, where the strata have not been disturbed, these roots can still be seen with the concretions around them.

In the Nankau pass, and for some distance about the place where it opens out to the plain, there appear large quantities of transported boulders. These were probably borne near to the places they now occupy by an old glacier that once filled up this pass, and brought them down from the neighboring mountains, or perhaps even from the southern borders of the high plateau of Mongolia, on which this river of ice probably took its rise. But a short distance from the mouth of the pass, in every direction over the plain, these boulders completely disappear. Many of them have been gathered by the farmers to make walls of their houses, but as few are to be seen in the clay banks, the question naturally arises, whether the materials that now fill the Peking basin have not been so completely sorted and resorted by the action of the waves as the land has rose and sunk from the level of the sea, that the larger boulders are mostly resting on the rocky floor of the basin, or at least at a considerable distance below the present surface of the ground.

As we followed the flanks of the mountains, southward, we came to a remarkable depression in the plain, evidently the bed of a lake that had recently been drained off, not over the plain toward Peking, but through some rent in the mountains toward the west, into the present channel of the Yang Ho, and along the course of this river to the gulf of Pechili. Farther down the Yang Ho, a small stream comes in on the southwest from a valley where are located the coal mines of Mun-to-kow. This minor valley is bordered with a terrace of 40 or 50 feet in height. Besides these evidences of the late presence of the sea in this region, I was shown at Peking, some shells from banks in the vicinity, and I believe they were all of the same species as are now to be found in the gulf of Pechili.

All these changes have occurred without especially attracting the notice of the people, but it has been far otherwise with the Yellow river, whose irregular wanderings and destructive floods have gained for it the well merited title of "China's sorrow."

All rivers after they have worn out their channels to a certain depth, have a tendency to deposit in their own beds a part of the mud and sand they are bearing along, and this tendency is greatly increased by preventing them from overflowing their banks by artificial levees or dikes. The Po in this way has raised its bed until the surface of its water is above the tops of the

houses of the peasants, and it has already once deserted its old channel and formed a new one, and this is in short the whole history of the Yellow river. When it had filled up tis old channel to the south of Shantung, and succeeded in making a breach through its artificial banks, it followed very nearly its previous course north of Shantung to the gulf Pechili. The whole plain through which it flows in the lower part of its course being of alluvial origin, and completely intersected by streams and canals, its waters would readily find a lower channel which their momentum in coming down from the higher level of their old bed would enable them to quickly enlarge. The elevation of the land along the sea coast at the rate of five feet a century, would have a tendency to render its current more sluggish, and consequently the quantity of sediment deposited in its own bed, greater than if it remained stationary or was somewhat subsiding. But this tendency may have been partially counteracted by an equal or greater relative elevation of the area along its upper course, and it is perhaps worth remarking in this connection, that one of the latest changes that has taken place where the Asiatic continent joins that of Europe has been one elevation and that the Aral Sea, and the lakes east of it being merely remnants of one great internal, depressed sea, whose bed has probably undergone a considerable elevation.

The last change in the course of the Yellow river occurred when the Taiping rebels were approaching and threatening Peking, and is supposed to have been caused by a breach made either by them, or as is more probable, by the Imperialists, to arrest the progress of their formidable enemies. All accounts agree that this change is complete and that its old bed is dry, but this is merely another way of stating as fact what has just been assumed, namely, that the river continued in its old channel until that had become as high or higher than the surrounding country.

Dr. Martin of Peking informs me it now leaves its old channel a short distance below the city of Ifung, and passing northerly near Tsau-chau and Poh to Fam, it flows in a northeasterly direction to Tungping, and thence to Tsinan fu, the capital of Shantung, and down the bed of the Tatsing river to the gulf of Pechili. This course is just about at a right angle with the old one to Hwaingan; and the distance from its present mouth to where it emptied before into the Yellow sea is more than 380 miles in a straight line and more than twice that distance along the sea shore. Perhaps we can better appreciate how great a change this must have been, if we suppose that at some time the Mississippi, on reaching the city of Natchez, should sud-

denly desert its old channel to the Belize, and taking a more easterly course, pour out its waters into Mobile bay.

Probably no other river within historic time has wandered so far and so frequently from its old channels as this Yellow river, but also probably, no other river on the whole globe flows out on to a plain of such wide extent at right angles to its own course, and at the same time of such a perfect and continuous level. The Imperial or Grand canal indicates how remarkably low and how uninterruptedly level the surface of this plain must be. No other country has ever had such an artificial water communication, but what other has such wondrous natural facilities for one of such a length, and like this at right angles to the courses of its two greatest rivers? While the Mississippi, the Ganges and the Nile flow out through many mouths, the Hwang Ho now confines itself to one, though a part of its waters appear to escape southward through the Imperial canal.

Farther southward, the region about the mouth of the Yangtse Kiang has also been lately raised, though it may now be in a state of rest. This change is clearly shown by the bank of recent shells, described by Dr. Lamprey in his paper on the "Geology of the Great Plain."

The mouths of the Yangtse themselves have changed greatly, and Tsung Ming island, which now has a population of *half a million*, did not *exist* in the fourteenth century.

The Tunting and Poyang lakes, which act as immense reservoirs for this river, receiving a part of its surplus waters during the floods and pouring them out again during the dry season, are slowly filling up with the sediment brought into them by their streams on the south. It has been noticed, that near each of these lakes high mountains appear. This fact probably indicates the origin of their basins, for they are merely the lowest parts of large depressions, and where the strata have been so folded that the crest of the undulation is remarkably high, the depression of that undulation must be correspondingly low. This is what has occurred where these lakes and their neighboring mountains have been formed. On my way through the interior of China I crossed Tungting lake, and found its waters to be quite shallow, and that they spread out widely over the low, swampy lands along its southern shores in the time of the floods. When I reached Siangyin, I found the whole adjoining country overflowed, and that we were already standing by the lake, although according to the map, it ought to have been dry land for more than *twenty miles* farther.

The basins of these, and the other lakes scattered over the great plain, may be places of a slight local subsidence, but it

is far more probable that they are small areas not yet filled up with the alluvial deposits of which the whole plain is composed, and which has been brought down from the mountains mostly by the Yellow river and the Yangtse.

At Foochow and about the mouth of the river Min, I believe there is an area that has for some time been slowly subsiding. While all the other rivers in China flow out through a low delta, that they have formed themselves, the Min at once empties itself into the sea; no delta is seen, yet the Min *has* one, as much as the Peiho, the Yellow river, the Yangtse, the Tsientang by Hanchau and the Sikiang by Canton. Its delta consists in the small and dangerous banks about its mouth, and if the deltas of these other rivers were to subside as fast as, or faster than they are raised by the deposit of sediment over their surfaces, each would present a phenomenon strictly analogous to that of the Min. When I visited Foochow, I noticed these indications of a late subsidence, and Mr. Dunn, of Hedge & Co., kindly gave me these corroborative facts. In digging a well in their compound, "at a depth of from twenty-five to thirty feet below the surface of the ground, there were found two boards about four or five feet long and one foot wide, nailed at the ends to a post. At the same depth was found a quantity of broken crockery of the same kind as that now used by the lower classes of Chinese, and a number of pieces of half-decayed wood. The earth in which these things were found was a rather loose mixture of mud and sand, bearing a close resemblance to what is now seen along the river banks at low water. The impression upon my mind at the time, was that we had struck the remains of a Chinese house, and the workmen were of this opinion. The tide here has a rise of about 12 feet at high springs, which would place the post *about twelve feet* below the present *low water level.* According to the Chinese, what is now the navigable branch of the river (i. e., between the city and the foreign settlement) was some 900 years ago too shallow for junks and large boats. The south branch was then in general use. The long bridge at the head of the island (opposite the city of Foochow), and the remains of dikes and flood gates in that vicinity, lend an air of probability to this statement."

In the plain about Foochow, as indicated above, the river Min frequently changes its bed by washing away one bank and building up the opposite one. And at the foreign settlement a number of lines of stakes are placed in the edge of the river to catch this changing sand and clay, and gain land on that side. At first it might appear that this post with its boards had simply sunk on one side of the river channel and been

covered by the sand mud from the opposite bank, but the fact that it appeared to be a part of a house, and was found with pieces of crockery ware, shows it had probably not floated to the place where it was found. The simple fact that *twenty-five* feet of sand and mud had been deposited over these works of human hands, proves in itself their high antiquity, as plainly as in the case of the canoes taken out from a depth of twenty feet beneath the streets of the city of Glasgow.

In the south of China, Dr. Legge informs me that along the East river, he has seen a large bank of shells filled with specimens of shells which he believes will prove to be of living species.

Passing from the continent to Formosa, Mr. Swinhoe informs me that Castel Zelandia, a fort built by the Dutch in 1634, on what was then an island, is now some distance back from the river and in the *center* of the city of Taiwan fu; also that at Takao, recent crabs and recent shells are found at a height of 1,111 feet above the present level of the sea.

During my travels in the north of China, I came to Chefoo on the northern side of the promontory of Shantung, and Capt. Shufeldt, of the U. S. ship Wachusett, kindly took me over to the mouth of the Tatung river, near Sir James Hall's Islands on the west coast of Corea. That river was found to debouch into a large bay, and along the northern side of this bay is a gradually ascending plain, from which a mountain range rises up abruptly. An indication of the recent period in which this plain must have been raised above the sea, is seen in the fact that all the streams that flow over it from the valleys among the neighboring mountains have as yet only worn out for themselves shallow beds in the loose soil. The height of this elevation I estimate at from 300 to 500 feet.

Passing over to Niphon, we find on the western side of the bay of Yedo, a plateau some 200 feet high, its top as level as if made by the hand of man. A short journey from Yokohama back to Kanasawa, hence across to the bay of Kamokura, and up the Tocaido to Kanagawa, showed me that all this area is also of very recent marine origin. The highest place we passed over I judged to be from 500 to 600 feet above the present level of the ocean, but terraces up to twice that height will probably be found among the neighboring mountains. Most of the "hills" in this region have been formed by the streams washing out gulleys in the plateau. These have enlarged into valleys, and these again have widened until only sharp ridges are left of the once continuous plain.

In the bay of Kamokura is the island Inosima. At present it is connected to the main land by a sand spit, about half a

mile long, two hundred to five hundred yards wide, and from two to five feet above high water level. This I suspect is the island mentioned under the name of Kamokura, by Kaempfer, in the year 1691, while on his way from Nagasaki to Yedo. He describes it as follows:* "Off the shores of the outer bay of Jedo, was seen the Island of Kamokura, with high rugged shores, but of which the surface was flat and wooded. It was not above four miles in circumference, and was used like several other islands, as a place of confinement for disgraced noblemen. There being *no landing-place*, the boats that carry thither prisoners or provisions must be hauled up and let down by a crane."

North of Niphon, on the island of Yesso, terraces line the northern shore of Tsugar strait and Volcano Bay, and what has already been described in Corea, again appears there, but on a much grander scale. The greatest height to which I have been able to trace the recent action of the sea, is 1,180 feet. This was found on the flanks of the mountains north of Hakodadi, but when foreigners are allowed full liberty to travel where they please over the Japanese Islands, similar phenomena will undoubtedly be found at a still greater height.

As I passed up the gulf of Tartary, I touched twice on the western shore of the island of Saghalien, and continued to find terraces to a considerable height. Again, the village of Nikolaifsk, at the mouth of the Amoor River, is situated on a bluff that appears to have been recently elevated from 30 to 50 feet above the present level of the river.

All these facts considered in connection with the dry beds of friths and bays along the Siberian borders of the Arctic ocean, and the remnants of the old gulf that once washed the eastern flanks of the Ural, give us some idea of how the Asiatic continent has increased her area within the later geologic times.

ART. XXIV.—*On the Palæotrochis of Emmons from North Carolina;* by Prof. O. C. MARSH, of Yale College.

IN 1856, Prof. E. Emmons announced the discovery, in the lower "Taconic" rocks of North Carolina, of some peculiar fossil corals, which he regarded as the oldest representatives of the animal kingdom on the globe. Believing them to indicate a new genus, he proposed for it the name of *Palæotrochis*, and gave a description and figures of two varieties which he con-

* Vide Hildreth's Japan as it was and is, p. 357.

sidered specifically distinct.* The forms described were found in Montgomery county, where they occur in great abundance in granular quartz and slaty quartzite, having a vertical range of about 1,000 feet. Prof. Emmons attached great importance to the fact that they occur in auriferous rock, and he hence inferred that the gold must have been deposited as a sediment. Shortly after the description of *Palæotrochis* was published, Prof. James Hall, in a letter to Prof. Dana, suggested that these forms were merely concretions, an opinion subsequently controverted by Prof. Emmons, who maintained their organic character.†

These supposed corals are usually lenticular in form, often resembling two short cones placed base to base. In size, they vary from one-fourth of an inch to two inches in diameter. The exterior is covered with irregular striæ or grooves, which extend from apex to apex. Many of the smaller specimens have at one extremity a rounded knob, and at the other a cavity.

The following figures, from Prof. Emmons' Report, represent the more common varieties, and are here repeated for convenience of reference.

While making some investigations during the past summer on the structure known as "cone in cone," the writer's attention was called to these peculiar forms, and their inorganic nature was at once suspected. An examination of the interior of several specimens clearly indicated that they were not corals, and, as soon as microscopical sections could be prepared, they were more carefully examined, but no trace of organic structure could be detected, the entire mass being evidently a finely grained quartz. The specimens examined were undoubtedly authentic examples of *Palæotrochis*, as some of them, presented to the Yale cabinet by Prof. Dana, were sent to him by Prof. Emmons, and the rest were given to the writer by Prof. W. C. Kerr, the present State Geologist of North Carolina. It follows, therefore, that this name should in future be dropped from the genera of fossils. It is not, indeed, impossible, or even improbable, that the gold-bearing rocks of North Carolina may yet be found to be fossiliferous, but up to the present time no satisfactory evidence of their being so appears to have been brought to light.

* Geological Report of the Midland Counties of N. C., p. 62. Also this Journal, vol. xxii, p. 389.

† This Journal, xxiii, p. 278, and xxiv, p. 151.

Admitting the inorganic nature of these remarkable forms, their origin becomes an interesting question, and it certainly is not easy to give a satisfactory explanation of it. They appear, however, to have some analogy with "cone in cone," which, as the writer has shown elsewhere,* is probably due to the action of pressure on concretionary structure when forming. In some respects the two are quite distinct, but evidence of pressure is clearly to be seen in both.

If inorganic, these forms may, of course, be looked for in other formations, wherever the same conditions have existed. Prof. Newberry, of Columbia College, recently informed the writer that he once observed a somewhat similar structure in altered Tertiary rocks in Oregon. Nothing of the kind, however, except the Palæotrochis, appears to have been hitherto described.

New Haven, Ct., Jan. 25, 1868.

ART. XXV.—*On the Delta-Plain, and the Historical Changes in the course of the Yellow River;* by RAPHAEL PUMPELLY.†

THE extent of the great plain of Eastern China is pretty well known from native and Jesuit authorities. It lies in a semicircle around the mountainous peninsula of Shantung. Its outer limit, as approximately given on the Jesuit map, begins in the department of Yungping (fu), and, running west, keeps south of the Great Wall till Changping (chau) N.W. of Peking. Thence, remaining east of the southern branch of the Great Wall, it follows a general S.S.W. course, passing westward of Chingting (fu), till it reaches the upper waters of the Wei River. Here it turns westward into Hwaiking (fu), and crosses the Yellow river in that department.

From the right bank of this river it trends a little east of south, passing west of Jüning (fu) (Honan), and then turning eastward it continues south of Kwang (chau) and north of Luhngan (chau) in Luchau (fu). Here an arm of the plain, in which lies the Tsau lake, stretches southward from the Hwai river to the Yangtse, and continues eastward on the right side of this river, occupying the region between the river and Hangchau bay. A hilly region, in the center of which is Nanking, rises, like a large island from the plain, to the north of this arm.

* Am. Association of Science, Burlington Meeting, August, 1867.

† From Geological Researches in China, Mongolia, and Japan, during the years 1862 to 1863, by Raphael Pumpelly. 144 pp. 4to. Smithsonian Contributions to Knowledge, No. 202, 1866.

The Shantung boundary of the plain begins at Laichau (fu), and after describing a great bow to the south it turns west at Shukwang (hien), and running thence to Changtsing (hien), in Tsinan (fu), it turns to the south and around to the south-east. Keeping this course it remains nearly parallel to the Imperial canal till the Kiangsu frontier, which it follows to the sea.

The greater part of the area included within these limits is a plain which seems to descend very gently toward the sea, and to be very generally below the high water level of the Hwang Ho. It is the delta of the Hwang Ho, and in part also of the Yangtse Kiang, and is remarkable for its semi-annular shape, half inclosing, as it does, the mountain-mass of Shantung.

The city of Peking stands on a raised border of loam, sand, clay, and gravel, which forms the northwestern skirt of the delta-lowlands, and seems to extend southward fringing the mountains along its western side. The name of the Talo lake (Ta, great, and lo, plateau or raised plain) seems to refer to such a border, and in the article on Kichau in Yukung it is said that "the Lo (plateau) was drained."*

The fact, also, that in historical times none of the arms of the Hwang Ho have approached the western mountain border of the plain, both north and south of Kaifung, within a less distance than from ten to fifty miles, seems to point to the existence of a recent sea margin, which would be perhaps due rather to the detritus brought down by local streams than to the delta deposit of the Hwang Ho.

All the important changes in the lower course of the Hwang Ho have been recorded from early times by Chinese historians, and their documents and maps form the most complete history we possess of the wanderings of any river.

The Yukungchuchi (Peking, 1705), written by Chin Hu Wei, contains a series of maps in which these changes are laid down for a period of more than 3000 years. M. Biot has given the substance of that part of this work that relates to the Hwang Ho, in a carefully prepared paper.† I have, however, thought the subject to be one of sufficient interest to warrant the reproduction of the maps of Chin Hu Wei, with such explanations as will render them intelligible, without going beyond the limits of a work that is intended to give only my own contributions to the physiography of Eastern Asia.‡ For further information I must refer the reader to M. Biot's paper, of which I shall make use in explaining the maps.

* E. Biot, Sur le chapitre Yukung, Journ. Asiatique, 1842.
† Sur les changements du cours inférieur du fleuve Jaune, Journ. Asiat. 1843.
‡ The reader will have to refer to Mr. Pumpelly's volume for these maps.

In the Yukung, a chapter of the Shuking classic of Confucius, it is said that the course of the Hwang Ho was regulated by the Great Yu. Whether the works of Yu are to be understood as the labor of a single man, or as the results of the enterprise of a rising colony during several generations, there seems to be little doubt that more than 2000 years before the beginning of the Christian era the Chinese had brought this turbulent river under their control, by an immense system of dykes, and had begun to cultivate the extensive marshes of the delta plain.

Map No. 1 of the series, on plate 4, represents the course of the Hwang Ho as it existed, in the main, from the time of Yu down to 602 B. C.

Map No. 2 represents the course resulting from the first great change, that of the fifth year of the reign of Ting Wang (Chow dynasty), 602 B. C.

Map No. 3 serves to illustrate a passage in the writings of the poet Sse Ma Tsien, recording a diversion to the east and southeast. The easterly course, forming the Pien river, seems to have been the earliest recorded tendency of the river to follow its recent course. The opening of the first channels in this direction is given as occurring in 361 and 340 B. C.

The diversion, indicated on this map, through lake Yungtse to the southwest, happened, according to Sse Ma Tsien, toward the end of the Chow dynasty, during the third century before Christ.

Map No. 4 represents changes that occurred under Wutih (Han dynasty), about 132 B. C., when a great overflow toward the northeast took place, the river trending toward Kai (chau) in Chihli. At this time several arms were formed between Taming (fu) and the sea, which are also given. Previous to this, under Wentih, about 160 B. C., there was a breach formed at Yentsin near Kaifung.

Map No. 5 gives the second great change in the course of the "river of Yu," which occurred about 11 B. C., and was caused apparently by the blocking up of the channels leading to the Pei Ho.

Map No. 6 shows the channels as they existed during the Tang, and five succeeding dynasties, till the beginning of the Sung dynasty.

A note on the map of Chin Hu Wei says, "the course of the river remained the same from the time of Ming Ti (Tung Han dynasty) A. D. 70 . . . . . till under Jin Tsung, A. D. 1034, when a break occurred at Hunglung, and another, fourteen years later, A. D. 1048, at Changwu, and the river of the Han and the Tang was entirely destroyed. The map covers a period of 977 years."

Map No. 7 (Pl. 5) represents the courses, under the Sung dynasty, from A. D. 1048 to A. D. 1194, a period of 146 years.

Map No. 8 records the course during the Kin dynasty. All the former channels appear blocked up, and the river, after entering Lake Lo, near the summit-level of the present Imperial canal, is seen to flow off to the N.E. through the Tatsing river, and to the S.E. through the Sz river. Lake Lo appears from the observation of Clark Abel, and from Chinese measurements, to be about 150 feet above the sea.

Map No. 9 shows the condition of the river under the Yuen and Ming dynasties, together with the Grand canal, a condition which seems to have remained substantially the same till within the last ten or fifteen years.

In early times the Yangtse entered the sea by three arms called the Sankiang, i. e., "Three Rivers;" and Chin Hu Wei has given a map of these, founded on the opinions of early authorities. I have indicated them on map No. 1 of the series.

A glance at the nine maps of the delta courses will show how widely separated have been the limits of divergence of the arms of the Hwang Ho, within the past 3000 years. A mighty river, ever turbulent, subject yearly to an enormous increase in volume, an increase regulated rather by the amount of precipitation in the distant Kwenlun mountains, than by the local climate, it has ever been the terror of the countless millions through whose midst it flows.

From the earliest times an immense force has been at work to keep it from breaking through its dykes, or, when this has happened, to guide and retain it between new embankments. The quantity of solid material carried by the river and deposited along its course, is so great that its bed is rapidly raised, and appears to have been, before the last change, higher than the adjacent country.

Biot says, "it is certain that the bed of the river, from Hwaiking to the sea, is higher than the adjoining country."

Several times, during the great wars that have preceded the downfall of dynasties, this condition of the river has been turned to account as a weapon of offense. Breaking the embankments has been made to accomplish, almost instantaneously, by the destruction of hundreds of thousands of inhabitants, conquests that had been delayed by years of brave resistance.

From the earliest time of colonization on the delta-plain, the task of keeping the Hwang Ho within its bed has been the constant care of the rulers of China, both when the country was united under one man, and when it has been subdivided

into petty states. In the latter case in the treaties bordering on the Hwang Ho, the clauses regarding the regulation of that river appear to have been the most important and the most sacredly observed.

One of the most striking results of the official corruption that becomes general during the decay of a dynasty, is the breaking loose of this great stream as soon as the means for maintaining its embankments are misapplied.

The devastation caused by these overflows is awful beyond description. The loss of life is very great, and the destruction of the crops that form the means of support of millions, produces famine and the overrunning, by starving hordes, of the more fortunate districts of the adjacent country. The anarchy that rules in this struggle for life is almost beyond the conception of those who inhabit lands where the population is much below the capacity of the country, or which are easily reached by foreign supplies.

Within the last fifteen years one of these great changes has taken place, apparently from the same cause and with the same effect as above indicated. Instead of emptying into the Hwang Hai, or Yellow Sea, the Hwang Ho now has its mouth in the gulf of Pechele, which it enters through the Tatsing river. The old mouth of the river was found to be dry in 1858.

According to information furnished to the Rev. Mr. Edkins, by officials of the Board of Foreign Affairs at Peking, the principal break occurred at Fungpeh (ting) in Süchau (fu), the waters flowing away to the N.E. In Tsinan (fu), the capital of Shantung, the waters of the Tatsing river are increased to six times their original volume by the contributions of the Hwang Ho.

In 1863 the river had not yet determined a channel, but its waters were spread over large tracts of country, and the city of Wuting (fu), nearly sixty miles north of Tsinan (fu), was almost inaccessible.

The present course of the Hwang Ho is indicated, so far as known, on Map No. 10.

Owing to the great quantity of material brought down by this river, and to the absence of great oceanic currents, that might, if present, interfere with its deposition, the delta is rapidly increasing in size, and the adjoining seas are becoming shallower.*

Probably nowhere can the rate of growth of deltas be better studied than in China. Cities that were built on the delta plain of the Hwang Ho several thousand years since are still

* Barrow estimated the hourly discharge of sediment at 2,000,000 cubic feet.

in existence, together with the archives of their history. In the cases of those that were built near the sea, the distances from this are given ; and frequent mention is made of towns, mounds, and natural hills, washed by the sea, within historical times, which are now far inland.

Thus, in B. C. 220, the town Putai is said to have been one li west of the sea-shore, while in A. D. 1730 it was 140 li inland,* a yearly increase of 100 feet, more or less, according to the length of the li. Hienshuikau (on the Pei Ho, in long. 117° 32′ E.) is said to have been on the sea-shore in A. D. 500,† and is at present about eighteen miles distant, an increase of about 81 feet per annum.

Along the southern shore of the gulf of Pechele the yearly increase N. E. of Shukwang since B. C. 220, seems to have been not more than 30 feet.

The sea-shore, according to local tradition, was near the present location of Tientsin (fu) during the Han dynasty.

It is also recorded that under the reign of the Han, the Hwang Ho entered the sea at Changwu, near the present Tsinghai.‡

ART. XXVI.—*Test for Bromids;* by Surgeon J. H. BILL, U. S. Army.

IN conducting some investigations during the past summer, on the physiological relations of the halogens, it became absolutely necessary in the course of the experiments, to devise a ready and sensitive test for bromine in the presence more particularly of chlorine.

Any analyst will bear out the assertion—although the books make light of the matter—that it is a difficult thing to recognize traces of bromine in the presence of excess of the other halogens. Thus in the case stated above, it was found impossible to obtain by the ordinary methods of the books, a certain, and easy recognition of bromine, when chlorine was present.

The Fresenius test solution of auric chlorid produces in faintly acid solutions of alkaline bromids, a coloration ranging from dark orange red, to light straw color, according to the strength of the solution. Iodids must be out of the way. Chlorids, however, do not interfere in the least. The following is the best way of applying the test. Separate iodids by palladium, and after getting rid of excess of palladium, by sulphuretted

* Fangyuchiyau; Chihli. † Fangyuchiyau; Chihli. ‡ Ibid.

hydrogen, concentrate the solution to about twenty-five cubic centimeters. Select two test tubes of the same size and shape, and color of glass. Into one pour the solution suspected to contain bromid. Into the other pour pure water, adding perhaps a trace of chlorid potassium; add now to each test tube, a drop of chlorhydric acid, and then to each *one* drop of auric chlorid solution. On now comparing the two tubes particularly in the direction of their long axes, a yellow color will be observed in the tube containing the bromid, and made very manifest by comparison with the other tube.

The following experiment shows the delicacy of the test applied as above. One centigram of potassic bromid was dissolved in one thousand cubic centimeters of water. Thirty centimeters of this solution, compared with thirty centimeters of a very weak solution of potassic chlorid, gave a decided yellow color. This experiment was varied by dissolving a gram of potassic chlorid in two thousand cubic centimeters of water, halving, and adding one centigram of potassic bromid to the one half. Thirty centimeters of each of the two solutions now tested, gave ample evidence of the presence of bromid.

The mixed chlorid and bromid should be brought to the state of salts of the alkalis if necessary, by precipitating with argentic nitrate, thoroughly washing, and fusing with potassic carbonate. If sodic carbonate is used, the subsequent reaction with the gold test is not so decided.

A test for chlorid in the presence of bromid, as simple and delicate as the above, is much needed. The writer has sought long for it but in vain.

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Art. XXVII.—*Shooting Stars of November 14th, 1867.*

In addition to the observations upon the November meteors of 1867, given in the last No. of the Journal (pp. 78–92), the following have been received.

1. *At Dartmouth College* (lat. 43° 42′ 15″·2, long. E. 19^{m} 03^{s}), by Prof. C. A. Young.—The observations were made by myself, assisted by a corps of volunteers from the Junior Class and the Chandler Scientific School.

Four observers were kept in position facing the cardinal points, and relieved at intervals of about twenty minutes, as the weather was very cold (thermometer ranging from 14°·0 to 20°·0 F.), with strong N.W. wind, and three or four inches of snow on the ground.

These four observers kept a record of the number of meteors appearing in their respective quarters of the heavens, while my own attention was mainly directed to plotting upon one of Mr. Hoek's star-charts the paths of such as were determined with sufficient accuracy, and noting with a chronometer the instants of their disappearance.

Observations commenced at midnight, at which time the sky was covered with swiftly drifting clouds; the wind gradually died away, and at $3^h\ 45^m$ the clouds disappeared, leaving a perfectly clear sky.

Before 3 o'clock the number of the meteors was disappointingly small, partly on account of the clouds. About 3 o'clock they became more common, by 4^h they were numerous, and at $4^h\ 45^m$ (local time) when the display reached its maximum, they were countless, appearing in squads, sometimes as many as a dozen at once. After $4^h\ 40^m$ no regular count was kept by the four observers, but at intervals of 15 minutes I noted the time required to count a number, looking toward the constellation Leo, but including in my count all that I could see with my head in that position. The annexed table contains the results.

Time of Observation.	No. observed. N.	E.	S.	W.	Total.	No. pr. h'r.	Cloudiness.		
12 to 3 A.M.	34	46	31	7	118	39	0·7	130	4 observers.
3 to 4.	66	70	72	28	236	236	0·5	472	
4 to 4·15.	113	102	81	73	369	1476	0	1476	
4·15 to 4·30.	173	168	136	97	574	2296	0	2296	
4·30 to 4·40.	251	206	185	117	759	4554	0	4554	
Totals,	637	592	505	322	2056				

At 5 o'clock	50	were counted in	2 m. 20 secs.,	1286	1 obs'r.
At 5·15 "	50	" "	3 m. 40 secs.,	818	
At 5·30 "	25	" "	3 m. 50 secs.,	291	
At 5·45 "	10	" "	5 m. 50 secs.,	103	

The last column of the table contains the number per hour, after allowing for cloudiness. The last four numbers in the column would require to be rather more than doubled to bring them up to the standard of four observers.

I do not think the number seen at this place can reasonably be estimated lower than 5000, and that too in spite of moonlight so strong that, aided as it was by the reflection from the snow, one could easily read or write by it.

The radiant, as determined by a rough graphical reduction of the paths of 82 which were mapped, appeared to be about $1°\ 30'$ in diameter, with its center in Dec. $+22°\ 15'$ and A.R. $10^h\ 02^m$. A motion of this point from south to north was suspected, amounting to about $1°$ or $1°\ 30'$ in the 4 hours from 1 to 5. The observations, however, will need a more thorough reduction in order to settle this question.

Most of the meteors were small, perhaps one in ten being equal to a first magnitude star. At the time of maximum the large ones were in still smaller proportion. The finest one seen here appeared at $1^h\,57^m$ just N.E. of the radiant. It was considerably brighter than Venus, and left behind it a train of brilliant greenish light (very much like burning brass), about 6° in length, which endured *more than six minutes*. This train gradually changed its form, doubled up, and floated away like a little cloud toward the N.W., evidently carried by some atmospheric current in the upper air. Before 8 A. M. the surface current had changed to the same direction. During the last two minutes of its visibility it was impossible to distinguish whether the train was seen by its own phosphorescence, or merely like any other cloud, by reflected moonlight. The train of one other meteor endured 90 seconds, and five others from 30 seconds to a minute.

The trains of the conformable meteors, without exception I believe, all shone with *greenish* light.

An unconformable meteor which appeared at $5^h\,15^m$ formed a beautiful contrast to the others; it moved from Orion toward Canis Minor, very like Mars at its nearest oppositions in brilliancy and color, and left a train of the same hue. Its apparrent velocity was only about half that of the conformable meteors.

As a general rule the meteors disappeared simply by burning out, without any appearance of explosion; to this there were three or four exceptions, but no noise was ever heard.

The time of flight varied from $0^s{\cdot}1$ to $0^s{\cdot}7$, except in the case of the unconformable meteors, which moved more slowly. About a dozen of these were noted during the night.

2. *At Iowa City* (lat. 41° 40′, lon. W. 14° 40), by Pres. N. R. LEONARD.—A company of the students of the State University had been formed some three weeks previous to the 14th of November, for the purpose of watching for the meteor shower, and making observations upon it if it made its appearance.

The lookout was commenced on the morning of Saturday, Nov. 9, and continued each clear night till the evening of Nov. 13th, four persons keeping watch at a time, or one for each cardinal point of the compass. With the exception of the mornings of Nov. 9th, and Nov. 12th, no more meteors were visible than may be seen on any ordinary night.

On the evening of the 13th, the prospect for a clear sky was very poor. A sort of haze seemed to prevail and light clouds spread over the heavens; these dispersed slowly until toward midnight all had disappeared. The moon, then but

little past its full, arose before the hour for commencing operations, and under its light all stars below the third degree paled away and became invisible. I noticed the Pleiades then just beyond a lunar-halo, but so feeble was their light, that only by looking steadily in the direction in which they were known to be, could they be perceived. I do not believe that a meteor of the average brightness of the Pleiades would have been perceived.

The company of observers was divided into three sections of twelve each, to watch respectively, from 10 o'clock to 12, from 12 to 2, and from 2 to 4 o'clock. The position chosen for an observatory, was an octagonal room in the cupola of the University. In each side of this room there is a window having an exterior column on either side, so that when the sash and shutters were removed, eight observers, stationed one at each of these windows, would command the entire circuit without any considerable overlapping of their fields of view.

One person was stationed at each of these positions, and two others were placed in a reclining posture on platforms outside, to watch for any meteoric flights that might occur near the zenith. One man stood at a desk within to make a record of the observations, while the twelfth was to act as occasion might require.

The paper for the recorder was ruled thus:

Quarter of compass.	Direction of motion.	Conformity.	Time visible.	Arc of motion.	Remarks.

The following is the record of the numbers counted, and of their conformity or non-conformity in direction to a line proceeding from the constellation Leo:

Hour.	Conformable.	Unconformable.	Total.
10 to 11 - - - - - - - -	2	5	7
11 to 12 - - - - - - - -	6	7	12
12 to 1 - - - - - - - -	35	16	51
1 to 2 - - - - - - - -	102	17	119
2 to 4 - - - -	Nearly all conformable,		5,000

During the last two hours the number actually counted was 4,748, but through mistake on the part of the person stationed at the west window, he only continued to call out to the recorder the direction, &c., of the brightest, without keeping in his own mind the total number of those seen. The division of the time between 2 and 4 o'clock was not noticed, so that the last two hours have to be given together.

In regard to their conformability, as soon as Leo had attained an altitude sufficient to admit of careful observation, it was

found that nearly all the meteors seemed either to issue forth from a point marked on our globe by the star 97, near the center of the sickle in Leo, or that, if their courses should be traced back, they would intersect at that point. For a few minutes, about $3^h\,15^m$ A. M., we carefully noticed those appearing in this quarter of the heavens, and they seemed to indicate a radiant *point* at the star mentioned; at about $3\frac{1}{2}$ o'clock it seemed that the radiant was a *line*, or, at most, a *very narrow ellipse*, having its center at this star, and extending in the direction of the star *zeta* in the sickle.

The exact number of conformable meteors for the last two hours cannot be given. Out of 1,638 counted by three observers having their attention directed to this subject, only 22 were noted as unconformable.

At 20 minutes before 1 o'clock, it was found that one person could not record fast enough, and the unemployed man undertook the record for one half of the circuit.

At quarter past two, both recorders were unable to keep a full record and from this time forward each observer counted to himself the number appearing in his quarter, and only called out the most remarkable for record. Some time before 3 o'clock a second observer took his place at the S.E., and soon after a second upon the east. The result of their counting will be seen in the Quarter Table.

QUARTER TABLE.

Hour.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Total.
10–11	0	2	3	2	0	0	0	0	7
11–12	1	1	1	3	0	5	1	0	12
12–1	8	15	5	5	4	10	3	6	56
1–2	25	35	20	16	3	6	4	6	115
2–3				150		No count.			
3–4	693	635	721	1100	507	336	158	448	4748
Total,	727	687	750	1276	514	357	166	560	4937

The maximum of the display as to numbers was from $3^h\,15^m$ to about $3^h\,40^m$ A. M. During this period, other parties of nine each were formed and counted the stars issuing from the constellation Leo, finding on the average about 40 per minute. After $3^h\,45^m$, the diminution was rapid, as may be seen by this comparison. The number counted in five minutes being—from $3^h\,21^m$ to $3^h\,26^m$, 222; from $3^h\,41^m$ to $3^h\,46^m$, 135; from $3^h\,50^m$ to $3^h\,55^m$, 106; from $3^h\,56^m$ to $4^h\,01^m$, 72; and from $4^h\,03^m$ to $4^h\,08^m$, 50.

Those stationed on the southeast quarter, counted by hundreds from 3 to 4 o'clock, with the following result: Time of first 100 was 7 minutes; of the second, 5 minutes; the third,

6 minutes ; fourth, 3 minutes ; fifth, 3 minutes ; sixth, 3 minutes; seventh, 2 minutes ; eighth, 2 minutes ; ninth, 2½ minutes ; tenth, 4 minutes ; eleventh, 10 minutes—closing the count at $3^h\ 55^m$ A. M. It will be found by adding these times that they lost 7½ minutes. Most of this loss occurred after the eighth hundred, at $3^h\ 31^m$, when they were unable to count reliably because of the great number that appeared, so that their maximum would be about 3 hours and 35 minutes by their time, which was 7½ minutes too fast.

Of the 1,100, they considered that only 5 were unconformable.

Color of the trains.—Record was kept of 155 of the most luminous trains, with this result :

Green,	49	Very green,	9	Total,	58
Red,	35	Very red,	14	"	49
White,	24			"	24
Train and meteor of different color,					24
					155

As to the green there was some disagreement—some calling that blue, which others pronounced to be green.

In regard to the color, it is my impression that the proportion given by the above figures is not true, save for the brightest meteors—for of the hundreds that left trains behind them the greater number seemed to be of a green color — very many yellow at the middle and gradually changing to green at the margin—a color a little darker than the flame of the metal barium, and not far from the hue of copperas. On comparing this statement with the views of several observers, I find all agreeing thereto.

There were several particular observations taken. The first to be mentioned is, that in some instances a separation took place between the meteor and its train, before the former disappeared.

At 9 minutes to 3, a meteor started from the radiant and proceeded directly over the star *Dubhe*, (the northernmost of the two pointers), followed by a broad train about 5 degrees long. The separation of the meteor and train took place just as the former crossed the star named. The train remained visible for the space of 4 minutes, drifting meanwhile to the S.E., shortening up as it went, without, however, growing much, if any, narrower, and disappeared after retreating about 7 degrees. The last glimpses that I had of it gave me the idea of a spiral form, but I could not be very positive about it.

At 4 minutes to 3, another meteor left the radiant and proceeded to the star *Zeta Draconis*, leaving there a train, behav-

ing just as the last described, retreating 2 degrees in 3 minutes. At 3 minutes after 3, another passed over *Mizar*, (the middle star in the handle of the Dipper), leaving there a train that retreated 1½ degrees in 2 minutes.

At 8 minutes after 3, a very large meteor passed over to the head of Orion, leaving there a train 11 degrees long, and almost immediately afterward was seen to separate into several parts and disappear. The train floated a little to the east of south, a distance of 11 degrees, the middle moving more rapidly than the extremities, so that it took up the form of a crescent, with the horns pointing N.W. It should be noted that this train appeared near to the full moon, so that its period of visibility was thereby much shortened. From this train before the separation, one observer thought he saw three small black streaks descending for a short distance, and curved backward toward the bottom.

At 1 minute after 4, another meteor passed over to Sirius, leaving midway from Leo a train that exhibited the same movements as the last, giving the crescent shape before disappearance.

A change of course in the path of a meteor was carefully noted in one instance. A little after 3½ o'clock, a deep red meteor was seen passing very rapidly through Leo Minor, toward the N.W., describing an arc of 15 degrees, in ¼ of a second. About midway of its course it turned abruptly toward the west, at an angle of 15 or 20 degrees, with its previous direction.

Several bright meteors were seen to *flash out*, remain stationary, or nearly so, for an instant and then disappear. As a rule, it may be stated that the nearer these were to the radiant point, the less was their motion.

From a careful examination of the records made, there appeared to be difference in the *length* of the arc of flight, which has some connection with the quarter in which the meteor was seen. The following exhibits the average length for the last two hours:

Length of arc by quarters.

Hour.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.					
2–3,	16°	13°	6°	14°	28°	20°	17°	15°	Found from		107	obs'd	arcs.
3–4,	18°		11°	12°	23°	15°	16°	20°	"	"	125	"	"
Average for night,	17	19	10	14	20	17	19	18	"	"	350	"	"

The arcs described in the N.E., E. and S.E., are thus seen to be shorter than the average, but their *time of visibility* was a trifle *greater* than for other quarters.

In noticing the *rate* of motion, a difference will be found with respect both to the *hours*, and the points of the compass. Thus, by hours:

10—11,...16° per sec.	12—1,...19° per sec.	2—3,..21° per sec.
11—12,...15° "	1—2,...28° "	3—4,..19° "

By quarters of compass.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
20°	18°	20°	17°	25°	20°	21°	20°

Very few were timed during the last two hours, in the first four of the above quarters. From the few, we shall derive a series of numbers much smaller than the above. In the last four a considerable number were timed and the resulting-velocity arcs are greater, thus:

N.	N.E.	E.	S.E.	S.	S.W	W.	N.W.
12°	10°		17°	30°	22°	21°	23°

I will conclude this article with a table showing the direction of the flights of the meteors, arranged according to the quarters whence they issued.

Direction of flights commencing at 12 o'clock, 1 denoting 1st hour, 2 the 2nd hour, &c.

DIRECTION TABLE.

Origin of Meteor.		Direction of motion. N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
N.	1	1	1	--	--	--	3	--	1
	2	1	1	--	--	--	--	4	9
	3	2	--	--	--	--	--	13	1
	4	--	--	--	--	--	--	3	2
N.E.	1	--	4	--	--	--	1	5	2
	2	9	6	--	--	--	--	2	6
	3	3	3	--	--	--	2	--	1
	4	--	--	--	--	--	--	1	--
E.	1	--	1	--	2	--	3	1	--
	2	--	1	2	6	2	--	1	--
	3	1	9	5	4	--	--	1	1
	4	2	9	6	3	--	--	-	3
S.E.	1	--	--	--	--	2	2	1	--
	2	1	--	--	4	4	3	2	--
	3	--	--	1	--	5	12	--	--
	4	--	--	--	1	1	1	--	--

Origin of Meteor.		Direction of motion. N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
S.	1	--	--	--	--	--	3	--	--
	2	--	--	--	--	--	1	--	--
	3	--	--	--	1	--	15	1	--
	4	--	--	--	--	2	27	--	--
S.W.	1	--	--	--	--	1	--	6	2
	2	--	--	--	--	--	2	3	--
	3	--	--	--	--	--	6	3	--
	4	--	--	--	--	--	3	17	--
W.	1	--	--	--	--	--	1	1	--
	2	--	--	--	--	--	2	1	--
	3	1	--	--	--	--	2	9	--
	4	--	--	--	--	1	--	9	--
N.W.	1	--	--	--	--	--	1	1	2
	2	--	--	--	--	--	1	--	3
	3	--	--	--	--	--	5	4	6
	4	1	--	--	--	--	--	29	16

Totals for these columns:	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
	23	35	14	21	18	96	114	55

But few were described as to direction during the last hour, at the first four stations. However, enough is presented to show three things.

1st. That the greatest uniformity as to direction is to be found in those quarters farthest removed from the radiant.

2d. If the breadth of this table be increased two fold—and the sheet wrapped on a cylinder, the line of maximum direction will run around it from top to bottom, like the coil of a helix.

3d. The prevailing direction of motion is toward the west, or a little south of west.

3. *Observations at the U. S. Naval Observatory, Washington, D.C., and at Richmond, Va.*—From the published reports* of Prof. Newcomb, Prof. Eastman, and Prof. Harkness, to Commodore Sands, we give a few extracts.

Two sets of observations were arranged, one in coöperation with northern observers, and one in conjunction with Prof. Harkness who went to Richmond for the purpose of making observations for parallax. The first set was assigned to Messrs. Zumbrock, Titman, Lindenkohl and Main, of the Coast Survey, who assisted on the night of the 13th–14th. Prof. Newcomb observes that the trains were green upon their appearance, but changed to red and brown in a fraction of a second. He locates the radiant a portion of a degree north of that point of the line joining γ and ε Leonis which is one-third of the way from γ to ε.

He gives the following method of computing the heights of the meteors observed at Washington and Richmond.

"Out of 21 of the more brilliant meteors recorded by Prof. Harkness at Richmond, it appeared, from the identity of times, that 11 had also been observed at Washington. Of these 11, one was far south of Richmond, so that no reliable parallax could be obtained; and another seemed to be a mere accidental coincidence in time of two meteors, as they were seen in entirely different directions. The mode of obtaining the heights and distances is as follows:

"Mark on the celestial globe the path of the meteor as seen from each station, and continue the paths backward as great circles until they intersect. The point of intersection will be the radiant for that particular meteor. Take a plane perpendicular to the direction of the radiant as a fundamental plane. It will then be necessary to find the projections of the observing stations on this plane, which we shall represent by S and S_2, and also the positions, relatively to those projections, of the point in which the meteor path would, if produced, cut the fundamental plane, which we shall call P. To do this set the celestial globe so that the radiant shall be in the zenith. Then the horizon will represent the great circle in which the fundamental plane cuts

* *Observations and discussions of the November meteors of* 1867. U. S. Naval Observatory, Washington. Government Printing office, 1867. 8vo, 40 pp. and 3 maps.

the celestial sphere, which may be marked on the globe. The two meteor paths being now continued to the horizon, the azimuths of the points in which they meet the horizon will be the azimuths of the point P as seen from the points S and S_2 respectively. Call them A and A_2. Next, we want the azimuth of the line SS_2. To determine this, the terrestrial azimuth and depression of one station, as seen from the other, must be computed. Then rectify the globe for the latter station and for the moment of observation, and mark upon it the position of the other station as seen from it. From this point let fall a perpendicular Q upon the fundamental great circle. The azimuth of the point of meeting, measured upon the fundamental great circle, will be the azimuth of the line SS_2, which we shall call Z, and the distance SS_2 will be equal to the distance of the stations multiplied by the cosine of the perpendicular Q. Thus, in the triangle SS_2P, we have found the side SS_2 and the adjacent angles A—Z and $180°+Z—A_2$. The sides SP, and S_2P are then found by the solution of this triangle. The observing stations being projected on S and S_2 by lines parallel to the course of the meteor, it follows that *the sides SP and S_2P, are equal to the perpendicular let fall from the observing stations upon the straight line in which the meteor moves.*

"Let $\triangle$ and $\triangle^1$ be the angular distances of the meteor from the radiant at commencement and extinction, as seen from the first station, and $\triangle_2$ and $\triangle^1_2$ the corresponding distances as seen from the other station. Then the meteor and the points P and S will form a triangle right-angled at P, the angle at the meteor being $\triangle$ at first appearance and $\triangle^1$ at extinction.

Hence, we have—

$D = PS \operatorname{cosec.} \triangle$ = distance of meteor from first station at commencement.

$D^1 = PS \operatorname{cosec.} \triangle^1$ = distance of meteor from first station at extinction.

$D_2 = PS_2 \operatorname{cosec.} \triangle_2$ = distance of meteor from second station at commencement.

$D^1_2 = PS_2 \operatorname{cosec.} \triangle^1_2$ = distance of meteor from second station at extinction.

"Finally, the heights of the meteor above the earth will be found by multiplying these distances by the sines of the observed altitudes, correcting them, if necessary, for the effect of the rotundity of the earth. It will be seen that a separate value of the heights is obtained from the observations at each station. This is because more data are given by the observations than are necessary for the solution of the problem, and the solution is therefore so arranged as to give two values of

each of the unknown quantities. The agreement of these values will furnish a rough check on the accuracy of the observations.

"The actual length of the path described by the meteor, as given by the respective observers will be—

$$PS\ (\cot.\Delta - \cot.\Delta^1)$$

and $PS_2\ (\cot.\Delta_2 - \cot.\Delta^1{}_2)$

"If the observations agree, we have also—

$$PS\ \cos.\Delta = PS\ \cos.\ \Delta_2 + SS_2\ \tan.Q.$$

$$PS\ \cos.\Delta^1{}_2 = PS_2\ \cos.\Delta^1{}_2 + SS_2\ \tan.Q.$$

"From the fact that the errors of observation in the direction of the meteor track are, probably, many times greater than those in the perpendicular direction, it follows that this method of determining the heights and distances will be much more accurate than that in which no account is taken of the direction of motion.

"The present observations were discussed in the above way with a single modification. The precision with which the meteors appeared to radiate from the point in right ascension 151° and declination 22½° was such, that any deviation of the recorded tracts from a great circle passing through this point was presumed to be due to errors of observation. The most probable point of the track was supposed to be about two-thirds of the way from the beginning to the end, and the azimuths A are the azimuths of those points as seen from the general radiant."

In this way he obtained the following heights (in miles of 5280 feet) for 9 meteors.

No.	Height at beginning.	Weight.	Height at ending.	Weight.
2	72 miles.	7	30 miles.	12
3	108 "	5	55 "	9
5	74 "	6	47 "	9
6	82 "	5	38 "	9
7	90 "	6	50 "	10
8	104 "	4	41 "	8
9	115 "	5	58 "	9
10	79 "	7	41 "	11
11	198 "	2	65 "	5
Mean,	102 "		47 "	

Prof. Newcomb adds, "It appears from the observations that the mean height of the meteors, at first appearance, was 102 miles, and at disappearance 47 miles. The motion being oblique at an angle of 35° from the perpendicular, the actual mean length of the path of the meteors would appear to be 67 miles. But this length must be too great, since the meteors certainly went through their course in one second or less, probably in half a second. Their velocity being 44 miles per sec-

ond ; the mean length of path can hardly exceed 22 miles. This very great discrepancy is accounted for by the tendency of the eye to assign to a swiftly darting body, which suddenly appears and disappears, a range of motion much wider than its actual range, and especially to place its point of beginning too far back. Allowing for this I conceive that, on the average, *the brighter meteors first appeared at an average height of 75 miles, and were extinguished at an average height of 55 miles above the earth.* I would also remark that there is no positive evidence that any one meteor commenced at a greater height than 100 miles.

"I would also beg leave to submit some deduction respecting the probable number, magnitude, and nature of the meteoroids.

"During the thickest of the shower, meteors were counted at the rate of 3,000 per hour. In making this count it may be estimated that the observers saw all that fell within a circle of 150 miles in diameter, having its center 75 miles S.E. of Washington. From this number and the velocity of 44 miles per second, it would seem that in the thickest part of the stream traversed by the earth on November 13th, there was an *average of one meteoroid in 900,000 cubic miles of space.* In other parts of the stream they may, of course, be much thicker.

"Supposing three hours to be the usual time of a November shower, the thickness of the stream from north to south would appear to be 60,000 miles. The breadth is probably ten times as great, so that its transverse area may be estimated at 36,000,000,000 of square miles. The meteoroids are, therefore, distributed along it at the rate of 40,000 to the linear mile. The stream moving at the rate of 27 miles per second, it may be estimated that a million of meteoroids pass in a second. Since it takes the stream years to pass, we may roughly estimate that the total number of meteoroids in it may amount to a hundred million of millions. This is a mere rough estimate, which may be ten times too large or too small, the data being altogether insufficient for anything like accurate computation. But the data for estimating the magnitude of the particles are far more slender. Several of the meteors were so brilliant that Mr. Doolittle could see their reflection from the face of his chronometer in a bright moonlight, though their distance was from 100 to 200 miles. I myself compared the more brilliant ones to a Roman candle, seen at the distance of 200 or 300 yards. To throw so great a light to the distance of 150 miles *would require several thousand millions of common candles.* Giving so intense a light, it might seem that the body from which it emanates must be very large. But from the dynamical theory

of heat, and from the experiments of Prof. William Thompson and others on the increase of temperature experienced by a body moving through the air,* it appears that such a body is exposed to an increase of temperature represented in degrees of Fahrenheit by $\frac{v^2}{125^2}$, where v is the velocity of the body in feet per second. Hence, a meteoroid moving through the air with a velocity of 230,000 feet per second will be exposed to a temperature of more than 3,000,000° Fahrenheit, a temperature of which we see the effect in the intense brilliancy and almost instant vaporization of the body exposed to it. Assuming a mean weight of one grain, the entire mass of the stream may be rudely estimated as equal to that of a mass of iron 400 hundred feet in diameter.

"It is now known that the entire stream of November meteoroids follows Comet I of 1866, commonly called Tempel's comet,† in its orbit. Through the telescope this comet presents the appearance of a mass of vapor. Bodies possessing this appearance on the earth, such as steam, smoke, and clouds, are known to be composed of minute detached particles of dense matter. And no other known form of matter can present it, except an elastic gas. The comet in question cannot consist of an elastic gas, because there is no central nucleus of sufficient magnitude to prevent its instant expansion through space. From these considerations, I deem it probable *that Comet* I, 1866, *is simply an agglomeration of meteoroids, just dense enough to be visible in the solar rays.* And since other telescopic comets present the same appearance, it is probable that telescopic comets in general are formed of such agglomerations."

Prof. Harkness from 21 paths observed at Richmond, obtained for the place of the radiant R. A. 149°, Dec.+24°.

The determination of the mass of the meteors is still a dissideratum. Prof. Harkness gives an elaborate discussion of this subject, assuming that the light produced is always proportional to the amount of material consumed. This assumption is necessary, though known to be not strictly correct. It is admitted only because science has hitherto failed to furnish any better mode of estimating the mass of meteors.

The results of the computation are embodied in the following table, in which the columns, A, B, and C, are obtained severally by using constants derived from the burning of coal-gas in fish-tail burners, from the Drummond light, and from the electric light.

* Philosophical Transactions for 1860.

† This comet was also independently discovered by Paymaster H. P. Tuttle, U. S. N., at this observatory, on the 5th of January, 1866.

Light of Meteor.	Mass of Meteor, in grains. A.	B.	B.
10 Jupiter	2533·	108·6	8·114
Jupiter	253·3	10·86	0·811
Sirius	162·0	6·99	·522
1st mag. star	21·6	0·929	·069
2d mag. star	7·43	·318	·024
3d mag. star	3·71	·159	·012
4th mag. star	2·21	·095	·007
5th mag. star	1·46	0·063	0·005

"It is almost certain that the masses given in column C are too small, because, in order to obtain them, it would be necessary that the whole mechanical force of the meteor should be converted into light, without any obscure heat whatever. Under the circumstances such a result seems impossible. On the other hand, the masses given in column A are probably too large, because meteors burn at an immensely higher temperature than coal gas. The temperature at which the Drummond light is produced is among the highest attainable on the surface of the earth by any known process, and it probably approaches more nearly to that of the meteors than any other artificial means of illumination. For that reason, I think we may fairly adopt the numbers given in column B as tolerable approximations to the actual masses of these meteors.

"It will not escape notice that all the larger meteors leave trains behind them, and as the light of these trains is not taken account of in the formula given above, the masses, just found, should in strictness, be slightly increased; but the uncertainty in estimating the brightness is so great that the small error thus introduced is scarcely worthy of mention. Finally, notwithstanding all the uncertainty to which the method here employed is liable, yet it would seem that we may reasonably conclude *that the mass of ordinary shooting stars does not differ greatly from one grain.*"*

Prof. Eastman locates the radiant (by means of recorded paths), at R. A. 148½°, Dec. +22½°.

Up to $16^h\ 37^m$, one thousand meteors were counted, four observers being engaged a part of the time, and two the other part. From this time successive hundreds were counted by four persons in the following intervals; viz., 4^m, $5^m\ 30^s$, $5^m\ 35^s$, $5^m\ 45^s$, $9^m\ 2^s$, $9^m\ 37^s$, $10^m\ 31^s$, and $18^m\ 20^s$.

According to Prof. Eastman's observations, "the prevailing color of the trains was green, varied occasionally by orange and red. In several instances the middle of the train was green,

* The cloud left behind by the larger meteors fills several cubic miles of space. The mass of the meteor must be sufficient to create such a cloud. H. A. N.

and the edges were tinged a deep orange or red. Many of the trains remained visible several seconds, and sometimes even minutes after the disappearance of the meteor. The train of No. 52 was very brilliant, and visible about five minutes. It appeared to float toward the north as if driven by a steady breeze."

ART. XXVIII.—*On the occurrence of living forms in the hot waters of California;* by ARTHUR MEAD EDWARDS. (In a letter to the Editors, dated 49 Jane st., N. Y., Jan. 23, 1868.)

IN the May (1866) number of the Journal, were some notes by Prof. Brewer on the occurrence of living forms in the hot and saline waters of California, in which a slight error appeared, tending to mislead naturalists more particularly with regard to certain observations of mine. In the subsequent number for November, Prof. Brewer inserted a note making a correction in this matter, but, as the subject is one of much importance, I have taken the liberty of putting together a few notes relating thereto, and beg of you to insert them, at your convenience.

The facts in the case of the Californian water are as follows: Prof. Brewer was under the impression that I had found animal as well as vegetable organisms in several specimens collected by him during the prosecution of the State Geological Survey, and so wrote. I received but one specimen from hot or saline water, and that was gathered at the Geysers in water of a temperature of 120½° F. Unfortunately the rest of the collections made at this and similar localities did not come into my hands, but I have arranged so that I shall before long have specimens of this description, and doubtless the examination of them will throw much light upon the subject under consideration. Of the material I did receive, the amount was very small, and I made, as I had been requested, a very careful examination, with these results. I found it to consist mostly of fine sand, mixed with a little of what seemed to be the refuse of decaying vegetation, which we might easily understand would be blown or otherwise carried into the Geyser. Besides these substances, I found it to contain a very few frustules of Diatomaceæ; true aquatic plants. They are an *Orthosira*, most likely *O. crenulata* of Kützing which is the same as *Gaillionella crenulata* Ehr., and has been placed under *Orthosira orichalcea* W. S., and by Smith in his Synopsis. The number of frustules of this species is small, but enough for its determination. Besides this, I

found perhaps half a dozen frustules of a *Fragillaria*, most likely *F. capucina* Desm., which is synonymous with *F. rhabdosoma* Ehr. I also saw a fragment of a much larger species which looked as if it were *Cocconema lanceolatum* Ehr., but, as the piece was very small, I cannot be certain. There are also present some hollow hairs or spines which might have belonged to aquatic crustaceans, but are of a dark brown color, and therefore I am of opinion were derived from some insect and of extraneous origin. It will thus be seen that what I found in the single specimen I examined hardly bears out Prof. Brewer's remarks on the occurrence of living organisms in these hot waters. The only organized matters I detected were the siliceous loricæ of Diatomaceæ, which we have no proof were living in the water of the Geyser, and might, on account of their extreme minuteness, be carried from a distance, and the hollow spines or hairs which I am convinced are of insect origin. In connection with this matter and bearing upon it in a very close manner, it will be as well to mention here, and thus place upon record, one or two facts to which it may be desirable to refer at some future time. In the number for January, 1867, vol. iii, of Max Schultze's "Archiv für Mikroskopische Anatomie" is a paper by Ferdinand Colm of Breslau, entitled "Researches on the Physiology of the Phycochromaceæ Florideæ." Therein, besides mentioning many facts of interest to students of vegetable physiology, he states that certain *Oscillariæ*, namely, the *Beggiatoa* (one of which, *B. mirabilis*, bends and twists itself in a very remarkable manner, so that it produces vermicular waves and a motion looking like the peristaltic action of the bowels) which live in waters charged with sulphates at a high temperature, and hence, during the process of their growth, decompose the salt present and cause the evolution of free sulphuretted hydrogen. In the abstract of Dr. Colm's paper in the Quarterly Journal of Microscopical Science, the writer remarks that Dr. C. says, "since this group of algæ alone can flourish in hot and strongly saline solutions, it is probable that the first organisms which were present in the primordial sea which covered the earth, and was of very high temperature, if we may reason from the deductions of geologists, were *Oscillariæ* or rather *Chroococcaceæ*." Now in the hot springs of California there have been found *Oscillariæ* probably belonging to this order, besides *Diatomaceæ*. Prof. Whitney says, (Geology of California, vol. i, page 94), "Both the earth and the stream are highly charged with sulphuretted hydrogen and sulphurous acid, and the waters hold in solution a great variety of salts, especially sulphates of iron, lime, and magnesia; these salts as well as crystallized sulphur, are de-

posited over the rocks in the cañon, giving a peculiar and vivid coloration, which is perhaps the most striking feature of the place." This is also confirmatory of the supposition of the growth of plants of this kind in these springs, and it is easy to understand how the sulphur can be eliminated from the sulphates, or even the oxygen abstracted by the vegetation, during the period of its life, and sulphids deposited. In fact, the dark colored iron sulphid is particularly mentioned by Prof. Whitney as found in abundance at the Geysers. Furthermore, in the number of the London Quarterly Journal of Microscopical Science for July, 1867, is a paper by Dr. Lander Lindsey "On the Protophyta of Iceland," wherein he mentions that in the Geysers of that country grow *Confervæ* and *Diatomaceæ*, of which latter he enumerates seven genera, and says "the abundance of Diatoms in the thermal waters of Central and Southern Europe warrants us in expecting large additions to the Icelandic Diatomaceæ from this source alone." Now it would be of extreme interest to ascertain in what way and to what degree the saline and hot waters affect species of Diatomaceæ, as collections might be made in fresh water if it occurs near the hot springs.

Besides, these forms from the saline as well as from the fresh waters of the Pacific coast should be very carefully compared with those found in the immense deposits so common in that part of the world; one of which deposits Frémont found on the Columbia river, and others have been detected by the State Geological Survey of California in that State and elsewhere. The origin of these deposits, and all facts connected with them, are of especial importance at the present time. It must, at the same time, be remembered that the fact as to what constitutes a species in the Diatomaceæ is by no means settled, as less really is known of the life history of these minute organisms than of almost any other plants. Moreover, in the study of the Diatomaceæ and allied families, the observer has presented to him extremely advantageous opportunities of making himself acquainted with many points in the phenomena of cell-life in simple as well as more complex plants and animals. I therefore ask the coöperation of every one at all interested in the prosecution of science and the acquisition of knowledge, to the furtherance of this branch of study; and to such as are able and willing to collect I will furnish plain printed directions, and to all who desire to pursue this branch of investigation I will gladly furnish all the assistance in the shape of information and specimens in my power.

ART. XXIX.—*Topographical and Geological features of the Northwest Coast of America;* by T. A. BLAKE, A.B.

A FEW moments spent in the study of the map of the world reveal the fact that the coasts of the continents, in high latitudes both north and south, which are bordered by high mountain ranges, are peculiarly cut up and present a most intricate, and, if I may use the expression, ragged shore line.

As an instance in the northern hemisphere, we have the coast of Norway, bordered by the Scandinavian range, and in the southern, the eastern coast of Patagonia from the parallel of 40° south, bordered by the Andes.

The northwestern coast of America is no exception to the general rule, but shows, perhaps on a grander scale than in either of the examples cited, the result of the slow but mighty erosive action of glaciers, the formation of which is the natural result of atmospheric humidity, and a low mean annual temperature, due not only to geographical position but to high elevation.

The published and glaringly inaccurate charts of the northwest coast of America, north of the Island of Vancouver, fail to give an adequate idea of the vast archipelago of islands and net-work of channels with which the whole coast of British Columbia, and that of the lower and eastern part, below 59° N. lat., of our new territory Alaska, are fringed.

The straits of Fuca, lat. 48° 20′ (approx.), are the southern limit of this complex system of labyrinthian channels which afford peculiar facilities for inland navigation. They are bordered on the south by the high and snowy range of Olympic mountains in Washington Territory, the peaks of which attain an elevation of eight thousand feet above the sea. The serrated line of summits of this range and the ever varying shades and colors along its bold front, are a source of infinite pleasure to the lovers of natural scenery that may perchance behold them from Victoria on the opposite shore.

The western and northern shores of Vancouver are generally high, culminating in peaks 5,000 feet in elevation, while on the eastern and southeastern border it presents a large extent of gently undulating and productive land.

Victoria, the principal city, is at the head of a small inlet from the straits of Fuca, and although possessing many advantages of climate and location, is now languishing under the effects of the reaction due to its rapid and unhealthy growth during the great Frazer river gold excitement of 1858.

The shores in the neighborhood are rocky; the characteristic

rock consists of feldspar and laminated hornblende or pyroxene and is probably eruptive.

The most marked geological feature is the absence of rough surfaces and angular summits. The rocks are everywhere rounded, scooped and grooved. As might be expected, erratic boulders, the material of which is foreign to their present position, are of frequent occurrence, thus giving us another link in the chain of evidence which proves the special agency of glaciers in the peculiar configuration of the coast.

In the outskirts of the city, between it and the hill called Beacon hill, at the head of the small inlet which forms the harbor of Victoria, an ancient sea beach has been exposed by digging a ditch or drain through the alluvial land, the bottom of which I should judge to be at least twenty feet above tide water. The shells which occur there are, according to Mr. Harford, the conchologist of the Alaska expedition, all of existing species. The occurrence of this beach proves a comparatively recent upheaval, subsequent to the extensive denudation and erosion caused by glacial action upon the underlying rocks.

From the slight elevations in the rear of the city, a grand view of Mt. Baker, seventy miles distant, is to be had. This is the last of that line of volcanic peaks in our territory which commences with Mt. Shasta, and stretches northerly through Oregon and Washington Territories.

Its height, according to Mr. Geo. Davidson, Asst. U. S. Coast Survey, and Chief of recent expedition to Alaska, has been accurately determined, by triangulation probably, and is ten thousand eight hundred feet.

The climate of Victoria is mild and equable. A set of meteorological observations taken there during the years 1859, '60 and '61 shows the mean annual temperature to be about 50° Fahr. The coldest weather in 1860, being in January and February, when the thermometer indicated a range of from 47° to 22°, or 10° below freezing, and the warmest in June, the thermometer ranging during that month from 48° to 81.°

I am unable to give the rain and snow fall in inches, but a meteorological record kept by Dr. Chas. Forbes, Surgeon Royal Navy, at Esquimalt near Victoria, shows that during the year 1860, there were 185 fine days, 88 overcast and foggy, 96 rainy, and 4 days upon which snow fell. The rainfall is distributed throughout the year, and greatest in the winter months.

On the eastern shore of Vancouver, the Nanaime coal mines have been opened to a considerable extent and now promise to assume greater importance since the acquisition of the new territory, as the main supplies of coal for steamers going and coming will naturally be drawn from there. From various

sources I learn that the beds are much faulted, involving great expense in their exploration, and that they stand at a high angle. The shipments from these mines have been constantly increasing, and have probably reached a total amount of over two hundred thousand tons. The coal is probably of the same geological age as that of the Mt. Diablo mines, and is very similar to it in chemical composition, as shown by the comparative statement of analyses given by our state geologist in his report, (1st vol. geology). The difference, however, is in favor of the Mt. Diablo coal, that containing a smaller percentage of ash, though more water; why the latter is not as well suited for ocean steamers, in the absence of anthracite, as the Nanaime which is so extensively used, I do not understand.

The shores of Vancouver, and the smaller islands with which the inland waters to the east are studded, are lined with recent deposits, soft and friable, generally horizontally stratified and rarely over fifty feet above the water.

The most important river of British Columbia is the Frazer, which flows into the gulf of Georgia just north of the forty-ninth parallel, or the northern boundary of Washington territory.

The outlying islands afford an admirable barrier against the encroachments of the sea upon its comparatively broad, alluvial valley.

Above this river is a series of long and narrow inlets, often stretching inland for from forty to sixty miles, from one to two miles only in width, and walled with high and abrupt mountains, densely wooded with evergreen trees to an elevation, by estimate, of three thousand feet.

Butte Inlet, at the head of the gulf of Georgia, of which accurate surveys have been made by Capt. Richards, Royal Navy, is bordered by mountains from four to eight thousand feet in height, and there are places in these fiords within one-half mile and less of shore, where no soundings were obtained at three hundred fathoms.

When to such bold features we add the remarkable luxuriance of vegetation which covers these shores, and the innumerable cascades and torrents which leap from the mountain sides, a faint idea may be formed of the majestic and wild beauty of the scenery. It is indeed like sailing through a succession of Yo Semite valleys.

Sitka Island, in our new territory, is a mass of unexplored rugged mountains, many of which are capped with perpetual snow. The town is situated at the head of Sitka bay, the entrance to which is marked by the symmetrical volcanic cone of Edgecombe, which is the second of a series of volcanic peaks border-

ing the coast and culminating in Mts. Fairweather and St. Elias, the heights of which are variously stated. The latter is in clear weather plainly visible at a distance of one hundred and fifty miles.

The rock in the vicinity of Sitka is a grit, sometimes coarse, often passing into argillite. The trend of this formation seems to be parallel to that of the coast. It extends as far south as the Deep sea, a remarkable fresh-water lake twelve miles southwest of Sitka, on the opposite side of which syenitic granite occurs.

Limestone, highly crystalline, is found within a few miles of the town on the north. The vegetation and general appearance of the coast is very similar to that already described, though the beautiful Sitka spruce which is remarkable for its grace and the mathematical regularity with which its branches grow from the central stem, replaces the Douglass spruce of lower latitudes. Trees grow to large size, many from six to ten feet in diameter being seen.

Of course but little is known of the geology of the country. It is only along the shores that the rocks can be investigated; the roughness of its surface, and thick growth of timber and masses of fallen and decaying trees, covered with deep moss, always saturated with water, almost preclude geological observation. I have yet to learn of a man, white or Indian, crossing from one side of Sitka island to the other, a distance of not over twenty miles in some places.

The meteorological statistics kept for the Russian government show as a mean of twelve years' observations, the mean annual temperature at Sitka to be 42° 9′ (Fahr.), the extremes being very small. The same observations give a mean annual rainfall of eighty-three and three-tenths inches, the maximum being one hundred and five inches.

Along Chatham straits, east of Sitka island, the rocks are metamorphic, stratified mica schists, standing almost vertically, and showing a parallelism in their trend to the line of the coast and of upheaval.

Glaciers are common along the inland waters north and back of Sitka island; these often sweep grandly from the mountain gorges and reach to the water's edge, generally ending in a low crescent-shaped flat formed by the wash from their terminal moraines.

In Icy straits, north of Sitka island, the ice from them falls into the sea, and so great is the accumulation as to render navigation dangerous. In lat. 59° along Chatham straits, every marked depression has its glacier of greater or less extent. These glaciers are to be seen at points as far south as the mouth

of the Stachin river, and the lowest known limit on the coast is about 54°, at the head of the narrow inlet on the coast of British Columbia, east of Fort Simpson.

At the head of the peninsula of Alaska is the commencement of another great line of volcanic action which extends to the southwest, forming the peninsula and then, curving to the west, the long chain of Aleutian Islands—"stepping stones," as they have been aptly called, between the two continents—stretching far toward the Kamschatkan peninsula.

The rocks on the island of Kodiak, east of the Alaska peninsula, metamorphic sandstones and shales, also show a general parallelism in their trend to that of this line of upheaval—trending N.E. and S.W. instead of N.W. and S.E. as on the coast of the main land.

During the time spent by the recent expedition at Captain's bay, island of Unalaska, Dr. Kellogg, two of the officers of U. S. R. Cutter Lincoln, and myself made the ascent of Makushiuski Mt., a volcano on the northern end of the island.

A detailed account of the ascent may form the subject of a future article. From our observations the height of the volcano was determined to be, approximately, five thousand six hundred feet, that of the snow line, three thousand one hundred and sixty-eight feet.

No vegetation is to be seen above an elevation of two thousand five hundred feet, except that low form of vegetable life called "red snow" which occurred at an altitude of from four thousand to four thousand five hundred feet. An incipient glacier curves gracefully around through a gorge on the eastern flank of the mountain.

The island is marked by the entire absence of trees, though the hills are covered with a thick growth of grass. Its northeastern end is made up almost wholly of volcanic rocks.

Perhaps the most remarkable view of volcanic cones and peaks, snow covered, and rising from the sea, to be found in the known world, is that of Unimak Island, with its two volcanic peaks of Shishaldin and Pogromnaja, both conical peaks of unbroken symmetry rising to heights of between nine and ten thousand feet above the sea. Between them is Destruction peak, comparatively low and irregular in its outline, showing it to be a volcano in its early stages of development. In 1863 its eruption was the cause of the loss of many lives, whence its name.

The mountains are entirely snow covered and between them are vast intermediate snow-fields; it is only along the shore that the rocks are bare, and that apparently in very limited areas. The angle of side slopes of Shishaldin is about 35°.

Exaggerated ideas have been formed of the *known* mineral wealth of Alaska.

Gold has never, as far as I can learn, been mined with profit within the limits of our new possessions, though it has been obtained in paying quantities on the Stachin river, one hundred and fifty miles from its mouth, in British Columbia.

The coal hitherto discovered is of uniform poor quality, being nothing more than lignite (excepting that found on the Arctic coast), and is of the most recent geological age. The coal on Ounga island is exposed in a bluff rising two hundred feet high from the sea. Our own discoveries may lead to something better.

Magnetic iron ore is reported, but is not known certainly to exist in any large deposits. Lead ore, galena, is also reported to have been found at various places.

Native copper is found on the Copper river, but the hostile character of the Indians in that locality has prevented its exploration.

According to Grewingk, fossils of the Carboniferous age occur at Cape Beaufort on the Arctic coast, of the Jurassic period on the east coast of the peninsula of Alaska, bay of Katmai.

Tertiary fossils occur on the island of Kodiak (Sacharof bay and bay of Igatskoi), at Pavlovsky bay, peninsula of Alaska, on the island of Unalaska on the N.N.W., foot of Mt. Makushiuski.

Mastodon bones and teeth are found at Escholtz bay, Kotzebue sound, and on the Pribulow islands.

Of the interior topography of the country little is known; recent explorations made by Mr. Dall, who has explored the Youkon river extensively, will undoubtedly give us more authentic and reliable information in regard to that than any heretofore available.

San Francisco, January, 1868.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the specific magnetism of iron;* by PLINY EARLE CHASE.* —In my communication on the numerical relations of gravity and magnetism (Trans. A. P. S., vol. xiii, p. 126), after adducing various evidences of a correlation that had been long suspected, I endeavored to obtain approximate valuations for the constant factor, K, which was introduced in the comparison of the tidal forces with the force of equilibrium. These approximations led me to "suggest the

* From the Proceedings of the American Philosophical Society, Nov. 15, 1867.

propriety of considering the element of density (or of its correlative, the square of the time of molecular diffusion), in connection with both A and M."

In the year after this suggestion was made, Dr. Menzzer announced, as an experimental result (Poggendorf's Annalen, Nov. 1865; P. Mag., xxx, 456), that "the magnetizing powers of two coils which give the maximum of intensity are as the square roots of their weights."

It therefore appears,

1. From Graham's and other well-known laws:
   Elasticity $\propto$ specific heat $\propto$ (wave velocity).^{2}
   Density $\propto$ (time of molecular diffusion).^{2}
   Weight $\propto$ (time of sonorous vibration).^{2}
2. From observations on terrestrial magnetism:
   Tidal differences $\propto$ (magnetic differences).^{2}
   Magnetic variation $\propto$ (time).^{2}
3. From Menzzer's experiments:
   Weight $\propto$ (magnetizing power).^{2}

This indirect confirmation, of a conjecture which was at first based on a plausible analogy, encourages me to hope that the following comparisons between molecular and cosmical kinetic values may help to explain the specific magnetism of iron.

According to Tredgold, iron may be elongated about $\frac{1}{14000}$ without permanent alteration of structure. Now the ratio, at the earth's surface, of solar terrestrial attraction, is about $\frac{1}{16400}$, and four times the ratio of the specific gravity of air to that of iron varies, approximately, between $\frac{1}{14000}$ and $\frac{1}{17000}$. Although this range of variation is somewhat more than $\frac{1}{4}$ of the least value, or about $\frac{1}{3000}$ of the total force, it is but little greater than Helmholtz found in the vibration-ratios for the first overtone of a series of tuning forks (5·8 to 6·6 times the fundamental), while it is much less than the daily fluctuation in the terrestrial magnetic force, which appears to be intimately connected with the joint action of atmospheric elasticity and solar differential-tidal attraction.

The coefficient of atmospheric specific gravity, 4, suggests the ratio of the length of a sonant aerial column to that of an equivalent sonorous wave, as well as the time of a complete oscillation of each magnetic pulse. During each vibration, from a maximum of condensation across the position of equilibrium to minimum, or *vice versa*, the effect produced by any constant force would be four times as great as during the half oscillation, from either extreme to the point of equilibrium. The ratios of wave-velocity to elasticity and density, and of revolution to distance from the center of motion, point to various experiments upon the relations of magnetic capacity to tenacity, in iron, and of magnetizing power to specific gravity or to specific heat,* in coils of different metals.

* The specific heats of iron, cobalt, and nickel are nearly the same, being each about 27 times that of hydrogen. Recently discovered cosmical affinities of hydrogen and iron, and the ratio between solar and terrestrial superficial gravitation, may, perhaps, sometime lead to the recognition of a significance in relations which would now be generally regarded as fanciful and accidental.

If such experiments should show any intimate connection between elasticity and specific magnetism (a result which it does not seem unreasonable to anticipate), some of the mystery in which an interesting physical fact is now shrouded will be happily dispelled.

2. *On a new synthesis of oxalic acid and its homologues.*—Berthelot has succeeded in converting acetylene, C_4H_2, into oxalic acid by the direct addition of oxygen,

$$C_4H_2+O_8=C_4H_2O_8.$$

As the same chemist had already shown that acetylene may be directly formed from its elements, the synthesis of oxalic acid requires but two successive operations. The oxydizing agent employed is hypermanganate of potash, the solution of which is to be added to the gas at the ordinary temperature and the liquid gradually shaken as long as the color disappears. The oxyd of manganese separated is then filtered off, when the filtrate will be found to contain a large quantity of oxalate of potash. As carbonic and formic acids are produced at the same time, the author regards these as resulting from the decomposition of the nascent oxalic acid,

$$C_4H_2O_8=C_2O_4+C_2H_2O_4.$$

In forming oxalic acid, one volume of acetylene takes up two volumes of oxygen, and Berthelot remarks that this is the first instance in which a hydrocarbon unites directly with oxygen without the elimination of any element.

Ethylene is oxydized by hypermanganate of potash almost as readily as acetylene, carbonic and formic acid are produced, together with a considerable quantity of oxalic acid. In this case we have the equation

$$C_4H_4+O_{10}=C_4H_2O_8+H_2O_2.$$

Allylene, C_6H_4, by oxydation with hypermanganate of potash in the cold yields malonic acid, $C_6H_4+O_8=C_6H_4O_8$. Acetic and carbonic acids are formed at the same time as products of the oxydation of malonic acid.

Propylene, C_6H_6, also yields malonic acid, but in larger quantity than allylene,

$$C_6H_6+O_{10}=C_6H_4O_8+H_2O_2,$$

oxalic, acetic, formic and carbonic acids are formed at the same time by a secondary oxydation. Amylene, $C_{10}H_{10}$, appears to yield a mixture of pyrotartaric, succinic and malonic acids. Styrolene yields benzoic and carbonic acids. Oil of turpentine yields a resinous acid and a neutral volatile substance having the odor of camphor.—*Comptes Rendus*, lxiv, p. 35. W. G.

3. *On Methylic Aldehyd.*—Dr. A. W. Hofmann has found that a substance possessing the composition and properties of methylic aldehyd is formed when a current of air charged with the vapor of methylic alcohol is directed upon an incandescent platinum spiral. The bottom of a strong three-necked bottle of two liters' capacity is covered to the height of about five centimeters with moderately warm methylic alcohol. The first neck is provided with a tube

descending to the very surface of the liquid; into the second is fixed a loosely-fitting cork which carries the platinum spiral, the third neck communicates with a condenser the lower end of which is fastened into a two-necked receiver. This is connected with a series of wash bottles and the last of these with a water-jet aspirator by which a current of air can be drawn through the whole system. The spiral is then heated to redness and plunged into the bottle. After a few minutes, irritating vapors are formed and drops of liquid soon condense in the receiver. If the current of air be properly adjusted, the platinum spiral remains incandescent for hours, and even for days, and there is no difficulty in collecting from 50 to 100 grams of a liquid rather rich in methylic aldehyd. In lecture-table experiments, a table blowpipe may be used in place of an aspirator to keep the whole spiral incandescent. A harmless explosion sometimes takes place which merely blows out the cork carrying the spiral of wire.

The liquid in the receiver has all the properties of a solution of methylic aldehyd. When rendered slightly alkaline by ammonia, it yields with nitrate of silver, a silver mirror of "irreproachable perfection." When heated with a few drops of a fixed alkali and boiled, the liquid becomes turbid and deposits drops of a brownish oil, possessing the odor of ethyl-aldehyde-resin. From want of time, the author has not yet isolated and analyzed methylic aldehyd, but has established its presence by preparing and analyzing the methylic sulph-aldehyd, ϵH_2S_2 which is a beautiful white crystalline substance fusing at 218° C., and subliming without decomposition. The author promises a more complete examination of the new aldehyd, which can hardly fail to prove a most interesting substance.—*Proc. Royal Soc.*, xvi, 156. W. G.

4. *On Fraunhofer's lines and on the violet portion of the solar spectrum.*—ANGSTRÖM and THALEN have communicated to the Royal Swedish Academy of Sciences a memoir on the part of the solar spectrum between G and H, which is accompanied by a map, and serves to complete the work left unfinished by Kirchoff. The authors find that with a single prism of 60° filled with bisulphid of carbon, and with a magnifying power of 40, all the lines upon Kirchoff's chart, the faintest not excepted, may be distinctly seen. The charts attached to their memoir were obtained with such a prism. To obtain a greater intensity of light in the violet portion of the spectrum, the authors employed as a condenser an objective by Dollond of about 90 millimeters aperture and 3 meters in focal length. The image of the sun as thus concentrated was allowed to fall upon the slit of the condenser. In the neighborhood of the lines H and H_1, a colored glass was employed to exclude all foreign rays. The measurements were made partly by a glass micrometer introduced into the eye-piece and partly by a filar micrometer used whenever it was possible to illuminate the wires. The lines beyond H were too faint to be distinguished by the eye. The drawings were made by Thalén by hand, but the scale employed is a little larger than that of Kirchoff. By employing a large inductorium

the authors obtained a much greater number of iron-lines than had been observed by Kirchoff, principally in the blue portion of the spectrum. When, in place of the inductorium, a galvanic battery of fifty jars was used with metallic points for poles, a much greater number of lines was seen. For the iron lines the authors employed pieces of metal 10 millimeters in diameter; for the manganese spectrum, the poles were of pyrolusite. The coincidences between the metallic and solar lines were obtained by the well-known method of examining both spectra at once, each occupying half the field of view. In the space between A and G, Kirchoff and Hofmann found 73 iron lines corresponding to as many solar lines; this number the authors have increased to 220. In the interval between G and H the authors observed 170 lines, making the whole number of iron lines about 460, to which 100 or 200 others would have been added had not the feebleness of the sun's light compelled the authors to suspend their researches. Angström and Thalén consider it certain from their investigations that manganese and hydrogen exist in the sun's atmosphere. To the three hydrogen lines already known to exist, they have added a fourth which corresponds to the most remarkable line between G and H. The authors do not find any evidence of the existence of new elements in the sun's atmosphere. They consider it probable that with a larger battery, a much greater number of iron-lines would be observed, and insist upon the importance of determining what lines are due to the earth's atmosphere alone.—*Les Mondes*, xv, p. 238. W. G.

[*Note.*—With reference to the use of a single prism of bisulphid of carbon, it may be remarked that the authors do not give the dimensions either of the prism or of the telescopes employed, and that the power of a spectroscope greatly depends upon its size, or, in other words, upon the quantity of light. Compare upon this point, the remarks of Sigmund Merz (Pogg. Ann., cxvii, 654). With a flint glass prism of ·60° and sufficiently large to admit a bundle of rays 43 lines in thickness, and with a collimator of 34 lines aperture, Merz succeeded in resolving the line D into five.—W. G.]

5. *Carbonylic Sulphid.*—This body, intermediate between $\mathrm{C\Theta_2}$ and $\mathrm{CS_2}$, has been long foreseen. Than has recently succeeded in forming it and in determining its properties. He first attempted its direct synthesis by passing carbonous oxyd $\mathrm{C\Theta}$, and sulphur vapor through a red-hot porcelain tube. A considerable quantity was thus produced, but it could not readily be separated from the excess of carbonous oxyd. Moreover it was decomposed again by the heat into its constituents. Recalling then the fact that cyanic acid, by taking up the elements of water, was decomposed into carbonic dioxyd and ammonia according to the equation

$$(\mathrm{C\Theta})''\mathrm{HN}+\mathrm{H_2\Theta}=\mathrm{H_3N}+\mathrm{C\Theta_2}$$

(in which cyanic acid is viewed as the imid of carbonic acid), Than saw that analogy required a similar decomposition for sulpho-cyanic acid thus:—

$$(\mathrm{CS})''\mathrm{HN}+\mathrm{H_2\Theta}=\mathrm{H_3N}+\mathrm{C\Theta S}.$$

Experiment confirmed this theoretic view. By the action of strong sulphuric acid, the sulphocyanates take up $H_2\Theta$ and yield the gas with effervescence. The method of its preparation is as follows:

Into a cooled mixture of five volumes concentrated sulphuric acid and four volumes of water, is placed as much potassic sulphocyanate as will allow the mass to remain fluid. The evolution of gas commences spontaneously; should it become too violent the flask may be cooled by placing it in water; and if toward the close of the action, it is too slow, a gas-flame may be put under it for a moment. Thus regulated, a constant stream of gas is obtained, which contains the vapor of water and of carbonic disulphid, as well as a trace of cyanhydric acid, and probably of formic acid. It is allowed to pass through three U tubes, the first of which contains cotton covered with moist mercuric oxyd to absorb the acids, the second, pieces of unvulcanized caoutchouc to remove the $\mathcal{C}S_2$,* and the third calcic chlorid to dry the gas. It is then collected over mercury, upon which when dry it has no action, even after many days. The moist gas however, covers the mercury in a few hours with a thin coating of mercuric sulphid.

As thus prepared, carbonylic sulphid is a colorless gas, with an odor not dissimilar to that of $\mathcal{C}\Theta_2$, but at the same time aromatic, recalling perhaps that of H_2S, though not at all disagreeable. It is soluble in its own volume of water, to which it communicates its odor. The taste of this solution is distinctly sweet, followed immediately by a peculiar prickly sulphur taste resembling that of H_2S and of $S\Theta_2$ together. After a few hours, however, the solution exhales a strong odor of H_2S. It is twice as heavy as air and may be poured from one vessel to another. It reddens litmus feebly, much more so than $\mathcal{C}\Theta_2$. When ignited it burns with a beautiful blue slightly luminous flame, yielding $\mathcal{C}\Theta_2$ and $S\Theta_2$. It takes fire very readily, even by a spark on a match. If the jar be held with the mouth downward, the gas burns completely; but if it be reversed, and a taper be introduced, the gas takes fire, while the taper is extinguished, and again relighted at the mouth of the jar. Under these conditions the combustion is incomplete and sulphur is deposited. A cold and dry beaker-glass held over a jet of the burning gas, shows no trace of moisture. With $1\frac{1}{2}$ vols. of oxygen, it forms an explosive mixture. Potassic hydrate absorbs the gas completely, though more slowly than $\mathcal{C}\Theta_2$. Dilute acids evolve H_2S and $\mathcal{C}\Theta_2$ from this solution, so that the absorption appears to take place thus: $\mathcal{C}\Theta S+(HK\Theta)_4=K_2\mathcal{C}\Theta_3+K_2S+(H_2\Theta)_2$. The potash solution gives a copious black precipitate with ammonio-argentic nitrate; no trace of cyanogen was found in the filtrate.

Neither chlorine nor fuming nitric acid have any action upon the gas at ordinary temperatures. Passed over heated mercury in a bulb-tube, no change is observed, though if long boiled in the gas a trace of mercuric sulphid is formed. When sodium is treated in

* The success of this device is so great that Than recommends its use in all similar cases. The $\mathcal{C}S_2$ is completely removed.

the same way, a white crust is produced at the common temperature, which by heating easily melts and becomes darker. At a low red heat the sodium ignites, burning with a brilliant light and leaving a black easily fusible mass, containing no trace of sodic cyanid, thus proving the absence of nitrogen in the gas.

When passed through mercuric-ethyl, at near its boiling point, a violent action takes place, pure metallic mercury without a trace of sulphid is separated, and a yellow liquid is produced, having a strong garlic odor. The author supposes it to be ethylic sulphopropionate.

If heated to low redness the gas is decomposed. This fact may be used to determine its composition as follows:—

Near the closed end of a U tube, a fine platinum wire is fused, stretching across the tube. This arm being filled with the gas over mercury, and its volume ascertained, the wire is maintained at a bright red heat by means of the battery. About the wire the gas is decomposed; thick heavy clouds of yellow sulphur-vapor fall down the tube till the decomposition is complete. After cooling, the residual gas possesses the original volume. It is odorless, does not render baryta-water turbid and burns with a pale blue flame; the product of the combustion renders baryta-water at once milky. It is hence carbonous oxyd.

	Milligrams.
Since, therefore, 1 vol. (22·33 c. c.) carbonylic sulphid weighs	60
and 1 vol. (22·33 c. c.) carbonous oxyd weighs	28
The weight of the sulphur in the gas is	32

The gas contains therefore one atom of C, one of O, and one of S, and its formula is COS.

Two determinations of its density were made by Bunsen's method. The first however was on a portion of the gas before the use of the tube containing caoutchouc; it retains therefore a trace of CS_2 vapor. The observed density is 2·1152 and 2·1046 in the two experiments; the calculated is 2·0833. 60 is therefore its molecular weight. These results were confirmed by direct analyses of the gas, made according to Bunsen's methods.

In the opinion of the author, this gas is quite widely distributed in nature. Because it is so easily decomposed by water into CO_2 and H_2S, however, it has generally been confounded with these substances. From some experiments of his own, Than thinks he he can assert almost positively that this compound is contained in the new and remarkable thermal spring of Harkány; and also in the cold sulphur-spring of Parád, both in Hungary. He sustains this view by the fact that the water freshly drawn has precisely the odor of the gas, which recalls, but cannot at all be mistaken for that of H_2S; though on standing a few hours, the sulphydric acid odor appears. For these reasons it is probable that it occurs in many other sulphur springs, and it can scarcely be doubted that it could be found among the sulphur-gases of volcanic action, and perhaps in those arising from organic decomposition.

It is recognized by the following reactions: 1st, Potassic hydrate deprives the gas or its watery solution, at once, of its peculiar odor. The alkaline solution effervesces with dilute sulphuric acid, evolving H_2S (distinction from C^2S). 2nd, in acid solutions of silver or cadmium it produces no precipitate; but upon the addition of ammonia, the respective sulphids appear (distinction from H_2S). 3rd, Sodic nitro-ferri-cyanid has no action in neutral or acid solutions; but an intense blue-violet color is developed by potash or ammonia. 4th, Iodid of starch is decolorized in a short time by this gas.—*Ann. Ch. Pharm., Suppl.*, Band V, 236, *Oct.* 1867. G. F. B.

6. *Manufacture of sodic carbonate.*—Kessler's patent process consists in heating salt, mixed with chromic oxyd (manganic dioxyd, plumbic or ferrous chromate may also be used) to redness in a current of steam, producing HCl and Na_2CrO_3. The mass is drawn from the fire, mixed with charcoal and again heated; by which Na_2CO_3 and Cr_2O_3 are produced. The sodic carbonate is extracted with water, and crystallized. The chromic oxyd is ready for a new operation.—*Bull. Ch. Soc.*, II, viii, 299, *Oct.* 1867.

G. F. B.

7. *Allotropic states of Metallic Arsenic.*—When arsenicum is sublimed in a stream of hydrogen gas, there is obtained, beside the sublimate of hexagonal crystals, having a metallic luster, a black vitreous amorphous mass, and also a fine gray powder. Bettendorf has investigated the conditions under which these are produced, and finds that they are dependent upon the temperature. When pure arsenicum is sublimed in a quick stream of hydrogen gas, the hexagonal variety is deposited nearest to the heated part of the tube, next amorphous black metal is thrown down, while the anterior portion of the tube is filled with a bright yellow smoke, which is deposited of a darker yellow color, and becomes finally gray. Under the microscope, this gray powder is amorphous, and consists of minute adherent spheres. Its density at 14° C., is 4·71, and it is easily oxydized by dilute nitric acid. Heated to 358° or 360°, it passes so rapidly into the crystallized variety (sp. gr. 5·72) that a portion of it is sublimed by the heat evolved. The author has not yet been able to preserve the yellow modification, as it passes rapidly into the gray.

The amorphous glassy black arsenicum is obtained when the vapor is cooled to 210° or 220° C. It is readily procured in crusts 5 or 6 millimeters thick, by subliming the metal in a stream of hydrogen, through a tube, one portion of which is bent in a U form, and maintained at 220° by an oil bath. Its sp. gr. is 4·71, and it passes at 360° into the crystalline variety with an evolution of heat. It is not easily tarnished, nor attacked by chemical agents, dilute nitric acid being without action upon it. It constitutes the arsenical mirror of the Marsh test.

The density of the hexagonal variety was found to be in three specimens prepared by different methods 5·728, 5·726, 5·727 at 14° C.—*Ann. Ch. Pharm.*, cxliv, 110, *Oct.*, 1867. G. F. B.

8. *Metallic Cerium.*—Wöhler has recently prepared this metal

by igniting the double chlorid of cerium and potassium with sodium. Metallic globules weighing 50 to 60 milligrams were thus obtained which consisted essentially of cerium. The color of cerium is between that of iron and lead; when cut and polished it has a strong luster. It is malleable and sectile. Its sp. gr. is 5·5 at 12° C.: but this is only an approximation. In the air it is slowly tarnished at ordinary temperatures, acquiring a bluish tinge. At 100° C. it decomposes water feebly, evolving hydrogen. It is easily attacked by acids; being converted by nitric acid into bright brown ceric oxyd. When heated in the air to weak ignition, it burns brilliantly to oxyd; but if suddenly heated more strongly, it burns with explosive violence, throwing about a brilliant shower of fire. In the pulverulent state it takes fire even at 100° C.

When the slag, from which the metallic globules had been separated, was treated with water, a glistening purple powder was left, which was purified by digestion with dilute chlorhydric acid. The dark-purple residue consisted of minute lustrous crystalline plates which upon analysis gave cerium 72·20, chlorine 18·38, oxygen 8·41=98·99; giving the formula CeCl+2CeO. [Or $Ce_3(\Theta_2Cl_2)$, the dichlorhydrin of di-meta-tri-ceric base].—*Ann. Ch. Pharm.*, cxliv, 251, *Nov.* 1867. G. F. B.

9. *Delicate test for alkalies.*—According to Boettger, the coloring matter of the ornamental plant so common in our gardens, called *Coleus Verschaffelti*, is readily turned green by alkalies. By digesting the fresh leaves in cold absolute alcohol to which a few drops of sulphuric acid have been added, for 24 hours, a solution is obtained from which paper of a fine red color may be prepared. The color is not affected by carbonic acid, and hence it readily detects the presence of earthy carbonates in natural waters. Its sensibility is very great; a piece of the paper held in the flame of illuminating gas, is at once turned green by the ammonia present.—*J. Pr. Ch.*, ci, 290, 1867. G. F. B.

10. *Method for the direct determination of oxygen in organic analysis.*—Von Baumhauer uses a combustion tube, 70 to 80 centimeters in length, in which he places: 1st, a column of copper-turnings 20 c. m. long; 2d, 10 c. m. of broken porcelain; 3d, 25 c. m. of cupric oxyd, in pieces of the size of wheat, strongly calcined (separating this from the layers before and behind it, by asbestus); 4th, the substance to be analyzed, placed in a small boat; and 5th, at a distance of 6 or 7 c. m. from this, another boat containing a weighed quantity of argentic iodate. This extremity of the tube communicates with two gas holders, from which either pure hydrogen or pure nitrogen can be supplied at pleasure. The operation is commenced by passing a slow stream of hydrogen through the tube, and applying heat to the copper turnings, to reduce any oxyd present. The hydrogen is then completely removed by a current of nitrogen, and the ordinary apparatus for the absorption of the $\mathrm{C}\Theta_2$ and the $H_2\Theta$ is attached. The combustion is proceeded with in the usual way, and when it is finished, the argentic iodate is gradually heated. The oxygen thus evolved, burns any remain-

ing carbon and reoxydizes the copper, reduced from the oxyd; any excess is retained by the copper-turnings. When the silver salt is entirely decomposed, the current of nitrogen is re-established. When cold, the absorption apparatus is weighed as usual. A fresh calcic chlorid tube is now attached, a current of hydrogen is sent through the apparatus, and the copper-turnings are heated. The oxyd formed by the excess of oxygen from the iodate, is now reduced; and the water which results is collected in the calcic chlorid tube. From this water, the amount of oxygen yielded by the iodate, in excess of that needed to burn the organic matter, is readily calculated. If this excess be subtracted from the whole of the oxygen evolved from the iodate (a known quantity of which was taken), the remainder is the quantity of oxygen required to burn the body, in addition to the oxygen which the body itself contained. If therefore from the whole oxygen found in the products of the combustion, viz., the $C\Theta_2$ and the $H_2\Theta$, this quantity be subtracted, there will remain the amount of oxygen in the substance analyzed. For example: ·452 grm. oxalic acid and 1·256 grms. $AgI\Theta_3$ were taken, ·443 grm. $C\Theta_2$ and ·090 grm. $H_2\Theta$ were obtained. The second portion of water weighed ·1495 grm., corresponding to ·133 grm. oxygen. As 1·256 grms. argentic iodate evolved ·2125 grm. oxygen, ·2125 − ·133 = ·0795 grm., the quantity of oxygen used to complete the combustion. In the ·443 grm. $C\Theta_2$ and the ·090 grm. $H_2\Theta$ there are ·4022 grm. oxygen, the whole amount needed to burn the body. Of this the argentic iodate furnished ·0795 grm.; hence the substance itself yielded ·4022 − ·0795 = ·3227 grm. oxygen. The ·452 gram of oxalic acid contains therefore carbon ·1208, hydrogen ·010, oxygen ·3227; which gives as the percentage composition carbon 26·73, hydrogen 2·21, oxygen 71·39 = 100·33. The calculated values are carbon 26·66, hydrogen 2·23, oxygen 71·11 = 100.

An equally ingenious process for the simultaneous determination of the nitrogen is also given.—*Archives Néerlandaises*, i, 179, 1866. G. F. B.

11. *On Glycerin and its derivatives.*—Buff has published a short but interesting paper upon the rational constitution of these bodies. Starting with the fact that glycerin must contain one alcohol-residue $C H_2-\Theta H$, because of the production of glyceric acid, containing the corresponding acid-residue, $C\Theta-\Theta H$ from it, by oxydation; and again, that the production of isopropylic iodid by Erlenmeyer, by the action of phosphoric iodid upon glycerin, demonstrates that at least one hydryl-atom is united to the middle carbon atom; and therefore that two formulas for glycerin are possible,

$$\left\{\begin{matrix} CH_2-\Theta H \\ CH-\Theta H \\ CH_2-\Theta H \end{matrix}\right. \text{ or } \left\{\begin{matrix} CH_3 \\ C(\Theta H)_2 \\ CH_2-\Theta H \end{matrix}\right. ; \text{ as well as } \left\{\begin{matrix} CH_2-\Theta H \\ CH-\Theta H \\ C\Theta-\Theta H \end{matrix}\right. \text{ or } \left\{\begin{matrix} CH_2 \\ C(\Theta H)_2 \\ C\Theta-\Theta H \end{matrix}\right.$$

for glyceric acid, Buff goes on to decide between them. If glyceric acid had the latter formula, then it is evident that by replacing the two atoms of hydryl (which are united to the middle carbon atom), one by H, and the other by Cl, a chlorpropionic

acid of the constitution $\begin{cases} \text{C}H_3 \\ \text{C}HCl \\ \text{C}O\text{-}OH \end{cases}$ would be formed. This acid corresponds to the ordinary lactic acid $\begin{cases} \text{C}H_3 \\ \text{C}H\text{-}OH \\ \text{C}O\text{-}OH \end{cases}$. Wichelhaus has, however, shown that glyceric acid treated with phosphoric chlorid does not produce this chlorpropionic acid, but one isomeric with it. Of course (assuming the equality of the four carbon units of attraction), this can only be the chlorpropionic acid corresponding to sarco-lactic acid, $\begin{cases} \text{C}H_2\text{-}OH \\ \text{C}H_2 \\ \text{C}O\text{-}OH \end{cases}$ whose constitution is $\begin{cases} \text{C}H_2Cl \\ \text{C}H_2 \\ \text{C}O\text{-}OH \end{cases}$. Hence the formula of glyceric acid must be $\begin{cases} \text{C}H_2\text{-}OH \\ \text{C}H\text{-}OH \\ \text{C}O\text{-}OH \end{cases}$ and that of glycerin $\begin{cases} \text{C}H_2\text{-}OH \\ \text{C}H\text{-}OH \\ \text{C}H_2\text{-}OH \end{cases}$. The action of phosphoric chlorid, then, substitutes chlorine for a hydryl-atom, belonging, not to an acid, but to an alcohol residue; producing a body different from the bromine-compound obtained by the direct action of bromine upon propionic acid. In this body, as Buff has shown, the bromine is attached to the middle carbon atom, giving the formula $\begin{cases} \text{C}H_3 \\ \text{C}HBr \\ \text{C}O\text{-}OH \end{cases}$, which may be compared with that of the chlorpropionic acid of Wichelhaus, given above.

To which of these forms do the chlorhydrins belong? Is the chlorine in monochlorhydrin attached to the middle or to one of the end carbon-atoms? This question Buff has answered by reducing monochlorhydrin by means of sodium-amalgam. A colorless, odorless liquid was obtained, boiling between 185° and 190°, corresponding precisely to propylenic glycol. Since this body by simple oxydation yields the ordinary lactic acid, the latter will fix its molecular constitution. In order to decide this point, the conversion into lactic acid was effected by means of platinum-black; and the lactic acid thus formed was converted into the zinc salt, and its character established. It is hence certain that in the action of chlorhydric acid upon glycerin, the chlorine replaces a hydryl-atom belonging to an external carbon-atom, thus:

Glycerin.	Monochlorhydrin.	Propylenic glycol.	Lactic acid.
$\begin{cases} \text{C}H_2\text{-}OH \\ \text{C}H\text{-}OH \\ \text{C}H_2\text{-}OH \end{cases}$	$\begin{cases} \text{C}H_2Cl \\ \text{C}H\text{-}OH \\ \text{C}H_2\text{-}OH \end{cases}$	$\begin{cases} \text{C}H_3 \\ \text{C}H\text{-}OH \\ \text{C}H_2\text{-}OH \end{cases}$	$\begin{cases} \text{C}H_3 \\ \text{C}H\text{-}OH \\ \text{C}O\text{-}OH \end{cases}$

In dichlorhydrin, a similar reduction would produce, according as the second atom of chlorine is united to the middle or to the outer carbon atom, propylic or isopropylic alcohol, thus:—

$$\begin{Bmatrix} CH_2Cl \\ CHCl \\ CH_2\text{-}OH \end{Bmatrix} = \begin{Bmatrix} CH_3 \\ CH_2 \\ CH_2\text{-}OH \end{Bmatrix} \qquad \begin{Bmatrix} CH_2Cl \\ CH\text{-}OH \\ CH_2Cl \end{Bmatrix} = \begin{Bmatrix} CH_3 \\ CH\text{-}OH \\ CH_3 \end{Bmatrix}$$

Propylic alcohol. Isopropylic alcohol.

This reduction, effected by sodium-amalgam, yielded a clear mobile fluid, boiling from 82° to 85°, corresponding in composition and properties to isopropylic alcohol. To confirm this view, it was oxydized by sulphuric acid and potassic dichromate, and yielded acetone. These bodies may be thus represented:—

Glycerin.	Monochlorhydrin.	Dichlorhydrin.	Isopropylic alcohol.	Acetone.
$\begin{Bmatrix} CH_2\text{-}OH \\ CH\text{-}OH \\ CH_2\text{-}OH \end{Bmatrix}$	$\begin{Bmatrix} CH_2Cl \\ CH\text{-}OH \\ CH_2\text{-}OH \end{Bmatrix}$	$\begin{Bmatrix} CH_2Cl \\ CH\text{-}OH \\ CH_2Cl \end{Bmatrix}$	$\begin{Bmatrix} CH_3 \\ CH\text{-}OH \\ CH_3 \end{Bmatrix}$	$\begin{Bmatrix} CH_3 \\ CO \\ CH_3 \end{Bmatrix}$

—*Ann. Ch. Pharm., Suppl. Band* V, 247, 1867. G. F. B.

12. *The organic base, Neurine.* (a.) *Its discovery.*—In October, 1864, Liebreich (Ann. Ch. Pharm., cxxxiv, 29) obtained from an alcoholic extract of the brain, a crystallized substance having the composition $C_{116}H_{241}N_4O_{22}P$, to which he gave the name Protagon. On boiling this body with baric hydrate for 24 hours, it was decomposed, and yielded glycero-phosphoric acid, two or more of the fatty acids, and a new basic substance which he called neurine (*νεῦρον*). He prepared this base by precipitating the solution, after moderate concentration, with plumbic acetate, filtering from the plumbic glycero-phosphate, removing the excess of lead by H_2S, evaporating to dryness with the addition of oxalic acid to expel the acetic acid, removing the oxalic acid by digestion with baric carbonate, and filtering. The filtrate which reacted strongly alkaline, was exactly neutralized with chlorhydric acid, evaporated to a syrupy consistence and treated with $PtCl_4$; upon the addition of absolute alcohol, a bright to orange-yellow precipitate was thrown down, which was filtered off, and washed with absolute alcohol. This precipitate is neurine platino-chlorid. In this form it consists of microscopic needles; but when crystallized from water, it appears in six-sided tables, heaped upon each other like ureic nitrate. Liebreich's analysis of this body yielded the formula, $(C_5H_{14}NCl)_2PtCl_4$. By H_2S the platinum is removed and the neurine chlorhydrate obtained, which crystallizes in silky needles, remarkably hygroscopic.

(b.) *Its rational constitution.*—More than two years after this, Baeyer (Ann. Ch. Pharm., cxl, 206, Dec. 1866) published some investigations upon neurine. He prepared this base by Liebreich's method, obtaining the chlorhydrate in hygroscopic needles. The platinum-salt separated in large yellow prismatic crystals, upon evaporating its aqueous solution in a vacuum. Alcohol threw down from this solution the same salt in yellow granular crystals; and upon long standing, still a third portion separated. These three products, however, gave different results upon analysis; (1) gave $(C_5H_{14}NOCl)_2PtCl_4$, (2) $(C_5H_{12}NCl)_2PtCl_4$, (3) $(C_5H_{14}NCl)_2PtCl_4$. This result led Baeyer to the opinion that neurine was a compound of two or more bases. In order to unravel its constitution, he heated a moderately concentrated solution of the chlorhydrate with strong iodhydric acid and some red phosphorus,

for some hours to 120° to 150° C. in a sealed tube. On cooling, a considerable number of large colorless prismatic crystals separated, which had the composition $C_5H_{13}NI_2$. The iodine in this compound existed in two states of combination, since upon the addition of argentic nitrate, only a single atom separated as argentic iodid. When digested with argentic chlorid, one-half the iodine was replaced by chlorine, giving $C_5H_{13}NICl$. Heated with freshly prepared argentic oxyd, both iodine-atoms were removed, and the body $C_{10}H_{26}N_2OCl_2$ resulted. These reactions suggested the similarity of this substance to a compound obtained by A. W. Hofmann by treating trimethylamine with ethylenic bromid, having the composition $C_5H_{13}NBr_2$. By acting upon this artificially prepared body with an excess of iodhydric acid, Baeyer obtained a substance, $C_5H_{13}NI_2$, identical in properties with the compound obtained from neurine. From this coincidence and that of other compounds, Baeyer concluded that neurine was a tri-methylized substance; and since the iodine-compound $C_5H_{13}NI_2$ is only $N(CH_3)_3(C_2H_4I)I$, the base itself must be either trimethyl-vinyl-ammonic hydrate, $N(CH_3)_3(C_2H_3)HO$, or trimethyl-oxethyl-ammonic hydrate, $N(CH_3)_3(C_2H_4[HO])HO$. He suggests that the variation in the analyses may be due to the presence of both these bodies. He also calls attention to the facility with which neurine evolves trimethylamine, as a possible mode of origin of this body in the animal organism. He remarks farther that this oxethyl group $C_2H_4(HO)$ exists in taurin; and thinks choline from the bile, and sinkaline from the white mustard-seed are probably identical with neurine.

(c.) *Neurine and choline identical.*—In March, 1867, Dybkowsky (J. pr. Ch., c, 153) published a paper upon the identity of choline and neurine. He noticed that when the aqueous solution of the platino-chlorid of either of these bases was concentrated in a vacuum over sulphuric acid, two kinds of crystals appeared. One kind were long-pointed, six-sided, orange-red crystals in thin tables; the others were yellow octahedrons; these latter were less soluble in water than the former. He considers the latter a decomposition-product of the former salt; and says it is formed more abundantly the more slow the evaporation. The purest crystals, which appeared entirely free from the octahedrons, were selected for combustion. And from the results, the formula $(C_5H_{14}NOCl)_2PtCl_4$, identical with Baeyer's first formula, was deduced.* Besides the

* Dybkowsky calls attention to Baeyer's description of his platinum-salt in "large yellow prismatic crystals," obtained by evaporation, and in "yellow granular crystals," by precipitation with alcohol; and says that the product thus obtained was not the pure neurine-salt, since this is not yellow, but orange-red, and falls, on the addition of alcohol, in amorphous flocks. The product of its decomposition, however, just described, crystallizes in yellow octahedrons, which may be so united together as to resemble a prism; but this crumbles readily to octahedrons. This too is the body which alcohol precipitates as a crystalline powder. [It is to be remarked that some of Baeyer's analyses sustain the formula $C_5H_{12}NCl$ for the chlorhydrate, which is $(CH_3)_2(C_2H_3)NCl$ or the vinyl compound; while others give $C_5H_{14}NOCl$, or $(CH_3)_3(C_2H_4[HO])NCl$, the oxethyl base. Since these bodies differ only by H_2O, may it not be that the prolonged evaporation changes in this way the orange-red oxethyl neurine base into the yellow vinyl compound?—B.]

fact that the physical and chemical properties of choline and neurine are precisely alike, Dybkowsky confirms their identity by quoting a statement from Naumann upon the coincidence of the crystalline form of the two platino-chlorids. The paper ends with some reasons for supposing that protagon exists in the bile, and is there the parent of the choline, as it is known to be of neurine in the brain-extract.

(d.) *Its rational formula established.*—In a more recent paper, BAEYER (Ann. Ch. Pharm., cxlii, 322, June, 1867) has published some farther investigations upon neurine. Concluding from his previous experiments, that the platino-chlorid was variable in composition (though in what way he does not state, not having seen Dybkowsky's paper apparently), he examined the precipitate, first obtained by Liebreich, produced by adding a solution of gold to a not too dilute solution of neurine chlorhydrate. This precipitate consists of yellow microscopic needles, difficultly soluble in cold but readily in hot water, being deposited when the solution cools in brilliant yellow needles; and on spontaneous evaporation in long well formed prisms. These gave upon analysis the formula $C_5H_{14}N\Theta Cl$, $AuCl_3$ the composition being quite uniform. The presence of oxygen excludes the vinyl hypothesis and fixes the constitution of neurine as trimethyl-oxethyl ammonic hydrate $(CH_3)_3(C_2H_4[H\Theta])N\left\{\begin{matrix}H\\ \\ \end{matrix}\right\}\Theta$; the chlorhydrate being $(CH_3)_3(C_2H_4[H\Theta])NCl$. Iodohydric acid at a high temperature converts this body into trimethyl-iodethyl-ammonic iodid, $(CH_3)_3(C_2H_4I)NI$. This iodid corresponds precisely with the bromid artificially prepared by Hofmann, and, as already stated, may be produced from it. On treating either the natural or the artificial iodid with excess of argentic oxyd, however, instead of replacing I by $(H\Theta)$ as was expected, HI is abstracted, and trimethyl-vinyl-ammonic hydrate $(CH_3)_3(C_2H_3)NH\Theta$, instead of trimethyl-oxethyl-ammonic hydrate $(CH_3)_3(C_2H_4[H\Theta])NH\Theta$, is produced. The oxethyl base is more easily re-converted into the iodid than the vinyl base. Hence it appears that while neurine can readily be converted into the vinyl compound of Hofmann, the reverse process cannot be effected; at least, with argentic oxyd; this is the more remarkable, since its lower homologue, the oxymethyl compound, is easily so converted. The gold salt of the vinyl compound has the formula $C_5H_{12}NCl$, $AuCl_3$; hence the salt previously described by Baeyer as $C_{10}H_{26}N_2\Theta Cl_2$ consists of two molecules of the chlorhydrate with one of crystal-water $(C_5H_{12}NCl)_2+H_2\Theta$. As the oxethyl and the vinyl base differ only by $H_2\Theta$, may not neurine be the vinyl compound with a molecule of crystal-water? To settle this question, Baeyer treated dry neurine chlorhydrate with acetylic chlorid. The gold compound of the body thus produced, gave on analysis the formula $C_7H_{16}N\Theta_2Cl$, $AuCl_3$; or rationally $(CH_3)_3(C_2H_4[\Theta C_2H_3\Theta]NCl$, $AuCl_3$; being neurine in which one atom of hydrogen is replaced by acetyl. The existence in this base of the oxethyl group, $C_2H_4(H\Theta)$, is thus demonstrated. Baeyer suggests its synthesis by the action of methylic iodid upon Wurtz's oxethylamine.

(e.) *Identity of neurine and sinkaline.*—In September, 1867, A. CLAUS and C. KEESÉ published (J. pr. Ch., cii, 24) some researches on the identity of neurine and sinkaline. For the preparation of the former, they extracted the finely divided brain with a mixture of equal volumes alcohol and ether, decomposed this extract directly with baric hydrate, and distilled off the alcohol and ether on the water bath, continuing the heating for 24 hours. 8 calves-brains, treated in this way yielded as much neurine as 18, treated by the method of Dybkowsky. The excess of barium was removed by $C\Theta_2$, the filtered fluid was evaporated to a syrupy consistence and extracted with alcohol. This alcoholic extract was acidulated with chlorhydric acid, and precipitated with an alcoholic solution of platinic chlorid. A voluminous yellow clumpy mass was thrown down which was dissolved in hot water and allowed to evaporate spontaneously. A more or less distinctly crystalline mass was thus obtained, which consisted of three substances. The larger portion was neurine platino-chlorid, which was separated from the other two bodies by fractional solution. One of these consisted of distinct octahedrons and dissolved slowly in boiling water, from which it again crystallized on cooling. This body the authors conclude to be potassium or ammonium platino-chlorid, though they were unable to obtain the crystals free from organic matter. The other body, very soluble in cold water, was not examined.

They confirm previous statements of the appearance of the crystals of the neurine-salt; and add that when to their aqueous solution, a small quantity of platinic chlorid is added, the crystalline form changes to a rhombic prism. This behavior is also characteristic of sinkaline; the authors therefore compared the neurine platino-chlorid, with specimens of the sinkaline-salt furnished by von Babo, its discoverer. The crystals of the neurine-salt, formed in presence of an excess of platinic chlorid, coincided completely with those of the sinkaline-salt, not only in form but in angular measurement.

The auro-chlorids were also found to be identical in properties. The salts of both bases behave alike on heating and evolve the characteristic odor of trimethylamine.

(f.) *Synthesis of neurine.*—On the 16th of Dec. last, WURTZ made a communication to the French Academy upon the synthesis of neurine. He first attempted its production by the method suggested by Baeyer, by treating oxethyl-ammonic hydrate with methylic iodid. But though successful, the yield was very small. Its direct synthesis was then accomplished by uniting trimethylamine with glycolic chlorhydrin, $\left\{\begin{matrix}C H_2 H\Theta \\ C H_2 Cl\end{matrix}\right. + \left\{\begin{matrix}C H_3 \\ C H_2 \\ C H_3\end{matrix}\right\} N =$ $\left.\begin{matrix}(C_2 H_4 [H\Theta])' \\ C H_3 \\ C H_3 \\ C H_3\end{matrix}\right\} NCl$. Five grams of trimethylamine were heated with ten grams of glycolic chlorhydrin in a sealed tube in a water

bath for 24 hours. The tube was then allowed to cool, when colorless prismatic crystals separated, which were easily soluble in hot absolute alcohol, from which, however, they were partially deposited on cooling. These crystals are trimethyl-oxethyl-ammonic chlorid, and are very deliquescent. Treated with auric chlorid, the double chlorid separates as a yellow precipitate, which is soluble in hot water, and crystallizes in characteristic needles on cooling. Wurtz has compared this auro-chlorid with the same body obtained from natural neurine and finds them identical. Platinic chlorid produces no precipitate even in concentrated solutions of the artificial chlorid, but the addition of alcohol gives a precipitate yielding 31·8 per cent of platinum, the theoretical quantity. Moist argentic oxyd converts this chlorid into the hydrate, yielding a strongly alkaline liquid becoming syrupy on concentration, and evolving an ammoniacal odor when heated.—*Chem. News*, xvii, 8, *Jan.* 3, 1868. G. F. B.

8. *Hydrocarbons of the Pennsylvania Petroleum;* by C. M. Warren, (from a letter to J. D. Dana, dated Brookline, 25 Feb., 1868.)—Your favor of the 24th instant has been received. It may be true that my formulæ for the bodies obtained from Pennsylvania petroleum require the corroborative evidence of the analytical results and vapor densities from which these formulæ were deduced in order to entitle them to be regarded as more trustworthy than those of Pelouze and Cahours. But I contend that the superiority of my process employed in making the separations, as evinced by the constancy of the boiling points, and uniformity of the boiling point difference for $\mathrm{CH_2}$ throughout each series, should go far to establish for my work the claim of superiority. I should have included the analytical results and vapor densities above referred to, had it not seemed more appropriate that these should appear in a subsequent paper, which I hoped to publish shortly after;—the special object of my first paper being to consider the relation between elementary constitution and boiling-points in homologous series of hydrocarbons. Moreover, before publication of those results, I desired to repeat some of the elementary analyses, by my new process, in the hope of obtaining more exact results which might help to solve the question whether my 1st and 2d series should be considered isomeric. The facts that I had already obtained, although sufficient (as stated in the foot note p. 167 of my memoir) to raise a doubt on this point, would not justify different formulæ from those that I published.

I will give below a few of my analyses and vapor densities, including those of one or two members of each of my three series. I trust these may suffice for the present to show that my formulæ are well grounded, especially those of the 3d series, these being more at variance with those of Pelouze and Cahours.

1st series.— Oil of boiling point, 61°·3,

Specific gravity at 0°,	0·676
Vapor density found,	3·053
V. d. required for C_6H_{14},	2·974
The results of analysis gave	C 83·88
	H 16·58
	100·46
Theoretical numbers on C_6H_{14},	C 83·72
	H 16·28
	100·00

2d series.— Oil of boiling point, 68°·5.

Specific gravity at 0°,	0·689
Vapor density found,	3·038
Results of analysis,	C 84·43
	H 15·81
	100·24

Theoretical numbers as above.

3d series.— Oil of boiling point, 195°·8.

Specific gravity at 0°,	0·782
Vapor density found,	5·480
V. d. required for $C_{11}H_{22}$,	5·327
Results of analysis,	C 85·33
	H 14·65
	99·98
Theoretical numbers on $C_{11}H_{22}$,	C 85·71
	H 14·29
	100·00

Oil of boiling point, 216°·2.

Specific gravity at 0°,	0·791
Vapor density found,	6·164
V. d. required for $C_{12}H_{24}$,	5·812
" " " $C_{13}H_{26}$,	6·292
Results of analysis,	C 85·74
	H 14·66
	100·40
Theoretical numbers on $C_{12}H_{24}$,	C 85·71
	H 14·29
	100·00
" " " $C_{13}H_{26}$, the same.	
" " " $C_{13}H_{28}$,	C 84·78
	H 15·22
	100·00

In estimating an analysis, I consider that the determination of carbon is, as a rule, quite liable to be slightly deficient; and that, on the other hand, the hydrogen is likely to be in excess. Viewing my analyses of the members of the 3d series in this light, especially with reference to the hydrogen, I think no chemist would doubt that these bodies belong to the C_nH_{2n} series, and not to the series of hydrids, to which they have been assigned by Pelouze and Cahours. We must of course depend on the results of analyses to settle this question. Then to determine the probable formula of a body of this class, its position in the series, we rely mainly on the vapor density.

I have adopted for the body boiling at 216° the formula $C_{12}H_{24}$, although the vapor density found is 0·352 above the theoretical number, agreeing more nearly with that of the next higher member of the series. Prominent among the reasons that induced me to give preference to the formula adopted, is the fact that, with this formula the discrepancy is in the right direction, while with the higher formula it would be quite the reverse; it being generally the case, especially with bodies of high boiling point, that the theoretical vapor density is below that found by experiment.

Pelouze and Cahours gave to their body boiling at 216°–218°, the formula $C_{13}H_{28}$, the theoretical vapor density of which, by my calculation, is 6·365; their determination of the same was 6·569.

I would simply add, that it is my intention to present to the Academy at its next meeting, the long delayed article, which will contain full details of the analyses and vapor densities of all the hydrocarbons that I have separated from Pennsylvania petroleum, and I will send the article soon after, for insertion in the American Journal of Science.

II. MINERALOGY AND GEOLOGY.

1. *The Carboniferous age of a portion of the Gold-bearing Rocks of California;* by WILLIAM P. BLAKE.—In a former No. of this Journal* Prof. W. H. Brewer, in noticing the reports of the Geological Survey of California, observes as follows: "We are now led naturally to consider the question of the age of the rocks of the auriferous series in California. They have been regarded as older than the Carboniferous by all writers on the subject, previous to the publication of the Reports under consideration. The announcement, therefore, of their later age has created unusual interest among geologists." Further on page 364, he cites the following statement from Prof. Whitney;† "We have not a particle of evidence to sustain the theory which has so often been brought forward that all, or even a portion, of the auriferous rocks are older than the Carboniferous,—not a trace of any Silurian or Devonian fossil ever having been discovered in California or indeed anywhere to the west of the 116th meridian."

* Volume xli, No. 123, May, 1866, p. 361.

† Report of the Geological Survey of California, vol. i, Paleontology, Preface.

These statements, in connection with others made by Prof. Brewer,* are to the effect that prior to the labors of the Geological Survey of California the gold-bearing rocks of that State were regarded as older than the Carboniferous (i. e. Devonian or Silurian) by those who had previously made geological explorations in the State, or had published upon the subject, and that it was reserved for the Geological Survey to show that the gold-bearing rocks were not of those older periods. This is an error, inasmuch as the writer of the present notice had several years before distinctly published his opinion that a considerable portion of the gold-bearing rocks of California were of the age of the Carboniferous, and had given reasons therefor. This opinion was coupled, also, with expressions of doubt of the existence of Devonian and Silurian formations in the gold-region, although the possibility of their presence was admitted. These assertions are sustained by the following quotations:

"It is probable that the beds of limestone observed in the gold region and in the passes of Sierra Nevada are of the age of the Carboniferous, and that a considerable part of the auriferous slates are of the same system. A recent examination of the rocks of New Mexico leads me to regard this as more than probable. It is also probable that a great part of the rock formation of the gold region will ultimately be found to be Devonian and Silurian, though as yet there is no evidence on this point except the similarity in their lithological characters to the auriferous rocks of the Atlantic slope."† Again, in the body of the same work in a summary of the principal geological features of the central portion of the gold region it is written (p. 276): "The next important formation is the white crystalline limestone which was found developed to such an extent at Columbia, Sonora, Cave City and the vicinity.‡ It attains a great width, is nearly vertical in its planes of structure, and appears to form a long belt trending northwesterly through Tuolumne and Calaveras counties. It probably extends much farther both north and south. It is believed to be parallel to and conformable with the great slate formation. At Abbey's ferry on the Stanislaus it was found in connection with mica-slate and granite. This rock is doubtless metamorphic, but as no fossils have been found in it, it is impossible to decide upon the period to which it should be referred. It is possibly *Carboniferous limestone*, this formation having been recognized in the northern part of the State, while we are yet without evidence of the presence of Silurian formations west of the great central chain of mountains."

The carboniferous rocks referred to in the northern part of the State were those recognized by Dr. J. B. Trask, and noticed in his Report of 1855. These were not perhaps identified as gold-bear-

* "On the age of the gold-bearing rocks of the Pacific Coast." This Journ., xlii, July, 1866.

† Report of a Geological Reconnoissance in California. 4to, 1858. Preface, p. iv. (Author's edition of the Geological Report in vol. v, of U. S. Explorations and Surveys)

‡ [This limestone is traversed in places by quartz veins bearing gold.]

ing. In the geological Report by Dr. Newberry the possibility of the gold region being Carboniferous is mentioned. He says: "from its relations to the limestone discovered by Dr. Trask near the base of Mt. Shasta, and which is of Carboniferous age, one may suspect it to belong to that era."* On p. 29 he observes: "If, as seems probable, the fossiliferous limestones of the mountains connected with Mt. Shasta shall prove to be continuous with the limestones of the Sierra Nevada referred to above, they will perhaps serve as a key by which to unlock the whole structure and age of the great Californian range."

In a descriptive notice by the writer of the gold veins and deposits of New Mexico, the following observation occurs: "It is remarkable that in one place at least, gold occurs in strata of quartzose sandstone probably of the age of the Carboniferous, and in great ferruginous beds rather than in veins."†

At the time the foregoing observations were made, it was the commonly accepted opinion that gold bearing rocks in general and especially in North America were of Silurian age. To venture to refer any portion of the California formations to a period more recent, and as late as the Carboniferous, was a bold step in advance, and if it was not initiative, it is at least one of the links in the chain of evidence which has led to the conclusion that gold cannot be regarded as characteristic of the rocks of any particular geological epoch and that it may be found in formations of any period where the conditions are favorable.

If in opposition to the evidence which has been presented, it is objected that there was no demonstration of the age of the rocks by fossils, or by tracing continuity, from known rocks; or further that it was merely stated as "probable," or "more than probable," it may be said in reply that as regards the portion of the gold-belt under consideration it is yet a matter of opinion, not of demonstration. The discovery of other localities of Carboniferous fossils in the northern part of the state, at Bass Rancho and Genesee valley has afforded additional evidence, and yet Mr. Brewer very justly does not positively assert the Carboniferous age of this belt southward, but says "all these limestones are of the same lithological character, and there seems little doubt that all the patches are of the Carboniferous age that lie in nearly a direct line from north of the Klamath river to Tahichipi valley a distance of over 500 miles.‡"

In the article by Prof. Brewer on the age of the gold-bearing rocks of the Pacific coast,§ he controverts the writer's statement that the Silurian age of the gold rocks of California has not always been assumed, by partially citing paragraphs from my Report. One of these paragraphs, it is said, "paves the way for priority of discovery should they [the rocks] actually prove to be Silurian by

* Pacific Railroad Reports, vol. vi, p. 22.
† Proceedings Boston Soc. N. H., vii, July, 1859, p. 66.
‡ This Journal, xli, May, 1866, p. 362.
§ Ibid, xlii, July, 1866, p. 116.

stating that the conclusions arrived at by Sir R. I. Murchison were confirmed by his (Prof. Blake's) observations in California."

This is a serious imputation, but is, however, without foundation as will be seen by reference to the passage indicated by Prof. Brewer. This passage does not refer in any way to the age of the gold-bearing rocks, but relates solely to the date of their impregnation with gold and to the detrital deposits as connected with it. Moreover the paragraph does not state that the "conclusions," as Prof. Brewer says, but that the *conclusion* was sustained, and this single conclusion is given in full, so that there is no possibility of mistaking which one of Sir R. I. Murchison's conclusions was intended by me. The following is the paragraph.*

"All the observations upon the auriferous deposits sustain the conclusion arrived at by Sir Roderick I. Murchison, after his explorations in the Urals, that the formation of gold is, geologically speaking very modern; that it is one of the most recently formed metals; the rocks being probably impregnated with it after the Miocene period, and but a short time before the epoch when the powerful and general denudations took place which destroyed the large extinct mammalia."†

This paragraph follows a description of the various forms of auriferous drift, and no reference whatever is made to the Silurian or Devonian age of the rocks.

The other statement, given in my preface, of the probability of the presence of Silurian formations, when fully quoted, as in the commencement of this article, bears internal evidence of the injustice of this imputation by Prof. Brewer. A full statement of the conditions of the problem required that the possibility of the presence of Silurian rocks should be recognized. It is not possible to affirm with certainty today that they are not represented in the broad metamorphic zone of the Sierra Nevada. The recent discovery of Silurian formations in the silver and gold region of Nevada, east of the Sierra Nevada, and comparatively near the latter, renders this question more interesting.

There are other statements and criticisms in the articles by Prof. Brewer, to which the writer takes exception, but without reply, as it is not the purpose of this article to discuss claims of priority upon any other point than the opinion in respect to the Carboniferous age of a part of the gold-bearing rocks.

New Haven, February, 1868.

6. *Memoirs presented to the California Academy of Sciences*, vol. i, part II. "*Principles of the Natural System of Volcanic Rocks*, by F. Baron Richthofen." 95 pp. 4to. San Francisco, 1868.—The author classifies volcanic rocks in the following orders and families.

Order first: Rhyolite; fam. 1, *Nevadite;* 2, *Liparite;* 3, *Rhyolite proper.*

Order second: Trachyte; fam. 1, *Sanidin trachyte;* 2, *Oligoclase trachyte.*

* Report Geological Reconnoissance in California, pp. 278–9. † Siluria.

Order third: PROPYLITE; fam. 1, *Quartzose propylite;* 2, *Hornblendic propylite;* 3, *Augitic propylite.*

Order fourth: ANDESITE; fam. 1, *Hornblendic andesite;* 2, *Augitic andesite.*

Order fifth: BASALT; fam. 1, *Dolerite;* 2, *Basalt;* 3, *Leucitophyre.*

The late date at which this paper is received, prevents any extended notice. The author shows that he is familiar with his subject through extended observations of rocks in place, and his memoir is an important contribution to this department of Geology. Some of the theories adverted to are at variance with views held by some eminent geologists, but the well known scientific character of the author, the value of his labors in a similar field before, and the amount of field observation and experience which he brings forward to support the conclusions arrived at, give them an especial weight and value. W. H. B.

3. *Report on the Geology of Nebraska.*—The final report of the Geological Survey of Nebraska, is already in an advanced stage of preparation. Mr. Meek, the Paleontologist, has already figured and described over 100 species of Upper Carboniferous fossils, making in all 11 plates of the sizes of those in the report of the California Survey. There are over 350 figures, some of which are European, to afford means of comparison of the American forms with those identified by Geinitz and others with European types. The whole work has been done in that careful, exhaustive manner, which characterizes all of Mr. Meek's labors. Twelve plates of fossil plants, Cretaceous and Tertiary, will accompany the report. There will also be a map and full illustrations.

The preliminary report of Hayden, written in the field, is printed in the annual report of the Commissioner of the Land Office, and will soon be distributed. It comprises nearly 60 pages, and treats quite fully of the geological, agricultural, and economical resources of the State, and calls the special attention of the people to the importance of cultivating forest trees. It is very desirable that these surveys should be continued until every territory west of the Mississippi is carefully examined and reported upon.

4. *Documents sur les Tremblements de Terre et les phenomènes Volcaniques des Isles Aleutiennes, de la Peninsule d'Aljaska, et de la Côte N. O., d'Amérique;* par M. ALEXIS PERREY. 132 pp 12mo.—Prof. Perrey of Dijon published in the years 1863 and 1864, memoirs on the earthquake phenomena of the islands of the western and northern Pacific ocean and Northwest America; and here he returns, in his memoir of the above title, to the subject of the Aleutian Islands and Alaska, regions in which this country has now a special interest. With his usual thoroughness and energy, he has cited facts from a large number of works, and some that are accessible to but very few readers.

5. *Note sur les Tremblements de Terre en* 1863 *avec suppléments pour les années antérieures de* 1843 *à* 1862; par M. ALEXIS PERREY. 214 pp. 12mo. Présentée à l'Académie royale de Belgique, le 4

février, 1865.—This extended memoir of Prof. Perrey contains a catalogue of the earthquakes over the globe of 1863 as far as any accounts of them have been published, with details where they could be obtained; and also additions to his former notes on the earthquakes of previous years back to 1843. These successive reports on earthquakes and volcanoes by Prof. Perrey are the result of a vast amount of labor, and are of great value to geological science.

III. BOTANY AND ZOOLOGY.

1. *Miocene Flora of the Polar Regions.*—We have heard much of late of the extensive collections of fossil plants from various parts of the arctic regions, which have recently been studied by Prof. Heer. We have not seen his Flore Fossile de Régions Polaires, but an article in the Bibliothèque Universelle, translated for the Annals and Magazine of Natural History, gives an interesting summary of the striking results.

The arctic fossil flora counts 162 known species, of which 18 are Cryptogamic, 9 of them Ferns of large size. There are 31 Coniferæ, 14 Monocotyledons, and 99 Dicotyledons proper. Seventy-eight of these are supposed to be trees, and fifty shrubs, making 128 ligneous species out of 162. The fossil Coniferæ of Greenland, Iceland, and Spitzbergen, no less than of Arctic America, mostly resemble existing North American species. There is a Spruce very nearly related to our White Spruce, now one of our most northern trees. *Sequoia* or Redwood, now represented in California only, in two species, had four species in the Miocene polar beds, three of which occur at the same level in Central Europe. One of these three, the most abundant tree of northern Greenland, but of wide range over both continents, is very nearly allied to the true Redwood, *S. sempervirens.* Another, abundant in Iceland, is as near the *S. gigantea.* A third, from Greenland, is intermediate between these two. The bearing of facts like these is obvious, as also that of the discovery of a fossil *Salisburia* in Greenland, the extant species being from Japan. The Cypress family is represented by *Taxodium* and the allied *Glyptrostrobus*, also by *Thujopris*, all now Japanese and North American. There were Chestnuts and at least 8 species of Oak in Greenland; one of them has been traced to northern Canada, and is the analogue of our *Quercus Prinus.* In Greenland also a Walnut, a Magnolia, with coriaceous leaves, and a Plum; in Spitzbergen a Lime-tree (*Tilia*); in Iceland a Maple and a Tulip tree! An Ivy has been found on the shores of the Mackenzie river, two vines in Greenland, and one in Iceland; all three approaching certain United States species. There were then, in these now icy regions, forests of various deciduous or evergreen broad-leaved trees, as well as Coniferæ of various forms, "vines and ivy entwined these with their branches, and beneath their shade grew numerous shrubs and elegant Ferns." A. G.

2. *Les Fleurs de Pleine Terre.* 2eme édition, 1866. *Atlas des Fleurs de Pleine Terre*, etc.; par Vilmorin-Andrieux & Cie. Paris,

1867.—The first edition of this choice little work was noticed at the time of its appearance. This second edition is considerably enlarged, one fifth being added to the length of the page, and the pages increased to nearly 1300. It enumerates and briefly describes, in popular language, but in a good degree scientifically, the principal hardy herbaceous plants in ornamental cultivation in France, and gives details as to their management and propagation, specifying their varieties, often with considerable fulness. To this work, the *Atlas*, now issued, is an invaluable addition. It is printed in a page of the same size, so that the two may be bound together, as two volumes,—the second part, beginning with p. 1009, going with the Atlas. This second part treats of the application of the plants, described in the first part, to the decoration of gardens, grounds, and dwellings; gives lists under various heads of those most desirable for different situations and circumstances, e. g., selections of Grasses, of sweet-scented flowers, of those ornamental in fruit, of hardy Ferns, with directions for a fernery, of rockwork plants, of aquatics, of plants which will grow well in shade, of picturesque plants, whether for blossoms or foliage, of conservatory or tender plants proper to be turned out for ornamentation of grounds in summer, of plants for ribbon-border or for masses of color; a calendar of the flowers of each month; and finally plans of gardens, parterres, and other ornamental grounds, with the plants to use; classes of border-flowers, &c., according to their colors; and a dictionary of terms, and a list of principal synonyms in English, German, Italian, Spanish, &c. Altogether the book if translated into English, even without special adaptation to our own climate or needs, would really be worth more, in our opinion, than all the gardening books we have put together. One like it would be needed, however, for trees and shrubs. But, returning to the Atlas. This has 1128 extremely neat wood engravings of as many plants, mostly 6 upon a page or plate, the page opposite the figures containing the explanations. The figures to be sure are miniatures, each compartment being only two inches high, and look more natural through the botanist's lens than under the naked eye; but they bear magnifying, as only well executed work would, and they are so chosen as to illustrate a very great variety of species and families. The work is published by the celebrated seed merchants who edit it, Vilmorin–Andrieux & Cie, of the Quai de la Mégisserie, Paris.

3. *The great Dragon Tree of Orotava*, Teneriffe, the King and Nestor of the Monocotyledons, is no more. A drawing of it made by Mr. Borda, one hundred years ago, was published about 60 years ago by Humboldt, who first drew scientific attention to this vegetable wonder. The late Mr. Webb published, in his history of the Canaries, figures of it taken after the storm of July 21, 1819, had destroyed half its crown; and it has recently been described, measured and photographed by Prof. Piazzi Smyth. He gave the trunk a girth of 48½ ft. at the lowest accessible part, and a height of 60 feet. Fenzi of Florence, who announces the demise of the tree,

makes the circumference about 78 English feet, but intimates that he was not able to measure it exactly. When he visited it, a year ago, "it was still in excellent health, its immense crown covered with innumerable panicles of scarlet fruits, and the huge trunk, although completely decayed in the interior, sustained vigorously the spreading mass of fleshy branches and sword-like foliage." He charges the destruction of this famous historical monument to the sheer carelessness of the Spanish authorities, who might have prevented it by keeping up sufficient props. That venerable trunk was, it is said, as hollow and about as large in the year 1402 as in recent times; and the hollow had even then been used immemorially by the Guanches for religious rites. The age of 6000 years which has been assigned to it by some of the "most sober naturalists" may not be very greatly exaggerated. Probably even the cavity was as ancient as the oldest of California Redwoods. A. G.

4. *Geological and Natural History Survey of North Carolina; Part III. Botany, containing a Catalogue of the Indigenous and Naturalized Plants of the State;* by Rev. M. A. CURTIS, D.D., &c. Raleigh, 1867. pp. 158, 8vo.—An important, neatly printed catalogue. Its greatest interest is in the Cryptogamic part, which occupies considerably more than half of the volume. The Flowering plants enumerated are 1873, the Flowerless 2924, of which 2392 are Fungi, i. e., of the order to which Mr. Curtis has devoted the greater part of his scientific life. Acknowledgments are made to Mr. Sullivant for assistance in arranging the list of *Musci* and *Hepaticæ*, and to Prof. Tuckerman, in that of the *Lichenes*. This assistance, it would appear, was rendered several years ago, as the arrangement does not in all respects represent Prof. Tuckerman's later views. The catalogue is, undoubtedly "much the most extensive local list of plants ever published in North America;" and, from the central position of North Carolina in the line of the Atlantic States, and from its including the most developed portion of the Alleghanies, it is very important to our botanists in the illustration of geographical distribution and range of species.

As regards the *Fungi*, that State may be supposed to contain nearly all the species of the Atlantic States. It is much to be wished that Dr. Curtis would now seriously devote himself to the elaboration of a Manual of the *Fungi* of the United States. Otherwise a vast amount of knowledge of these obscure plants may be one of these days lost to the world, and a great want long remain unsupplied. A. G.

5. *Genera Plantarum*, by BENTHAM and HOOKER.—The third part, completing the first volume of this most important work, and the Polypetalous series, was published late last autumn, and should have been announced before. We expected, however, to have prepared an extended notice of the volume, which we have as yet been unable to do. The work supplies excellently a great want, and will at once be in the hands of all scientific botanists. A. G.

6. *Illustrations of the Genus Carex;* by FRANCIS BOOTT, M.D. Part IV, Tab. 412–600. 1867.—This posthumous volume, issued

by Mrs. Boott as a tribute to the memory of her late husband, has just come to hand, and will be received as a great boon by the lovers of Carex, who still feel the loss of their leader and master. A full notice of the work will find a place in the ensuing number.

A. G.

7. *Botanical Necrology for* 1867, *concluded.*—The following articles were excluded from the last number, for want of room.

Dr. C. H. Schultz (Bipontinus), a distinguished botanist who devoted his attention almost exclusively to *Compositæ*, and has brought together an extremely rich herbarium in that vast family, died at his home in Deidesheim, Rhenish Bavaria, on the 17th of December last, at the age of 62 years. He had brought together a greater amount of the materials requisite for a monograph of the order, than are possessed by any other herbarium; but his publications, although numerous, are mostly in scattered papers.

Dr. Charles D. B. Daubeny, Professor of Botany and Rural Economy in the University of Oxford, Senior Fellow of Magdalen College, died, at Oxford, on the 12th of December last, in the 73d year of his age. He was Professor of Chemistry in the University, from 1822 down to 1855, when upon the reorganization and extension of scientific studies in the University—largely due to his own efforts and influence—he resigned that chair to the present incumbent. He was appointed Professor of Botany in 1834, and Professor of Rural Economy in 1840, when by the completion of the publication of the *Flora Græca* Sibthorp's endowment became available.

As chemist, geologist, one of the founders of the British Association, and an active promoter of natural and physical science in various departments, Dr. Daubeny deserves a more comprehensive memorial than can here be attempted. In botany his contributions are in the line of vegetable physiology and of classical learning. His first paper upon such subjects (but some years later than his volume upon volcanoes, issued in 1826), "On the degree of Selection exercised by the Roots of Plants," probably brought forward his name for the Botanical professorship, almost a sinecure at the time, and to which, when it fell vacant, the University authorities, as we have been told, were about to prefer some "worthy" person who was wholly ignorant of natural science. But the College of Physicians in London, which shared the patronage, insisted upon a scientific if not a botanical incumbent, and so secured the appointment of Dr. Daubeny. To qualify himself for this position he became a pupil of DeCandolle, and afterward of Lindley; and in 1836 he justified the appointment by publishing, in the Philosophical Transactions, his Essay upon the action of light upon plants and of plants on the atmosphere. He here established the capital fact that the luminous rays (and not the chemical or actinic, as has quite lately been assumed by prominent physicists) of the solar spectrum are those chiefly efficient in the evolution of oxygen gas from the green parts of plants, and therefore in assimilation. This has been followed by many other papers or lectures upon sim-

ilar subjects, mainly upon the applications of vegetable physiology and chemistry to agriculture, but none of such scientific importance as this. In 1857 he published his Lectures on Roman Husbandry, treating also of the Horticulture of ancient times, and concluding with notices of the plants mentioned in Columella and Virgil. In 1865 he published the substance of another course of lectures in a volume entitled, Essay on the Trees and Shrubs of the Ancients,—a work which was noticed at the time in this Journal. Dr. Daubeny did not pretend to be a practical botanist; but in his biographical notice of his preceptor DeCandolle, he shows that he had well mastered the principles of the natural system. The neat Botanical Garden at Oxford owes much to his liberality as well as his care; and a little subsidiary experimental garden, excellently planned, has been entirely supported by the resources of the venerable and much respected late Professor. Dr. Daubeny visited the United States in 1837 or 1838, travelled widely over the country, and printed for private circulation a brief account of his observations. His chair will be filled, we trust, by a better, because a special, botanist, but it hardly can be by a more excellent man.

A. G.

7. "*Memoirs presented to the California Academy of Sciences*, vol. i, part I. *A Catalogue of the species of Mosses found up to the present time, on the northwest coast of the United States, and especially in California;* by LEO LESQUEREUX." 38 pp. 4to. San Francisco, 1868.—As the title indicates, this is a mere Catalogue; but it contains descriptions of quite a number of new species. The enumeration comprises 265 species, 215 of which are from California. The most of these have been collected by Mr. Bolander, of the Geological Survey, and upward of 90 species have thus far been found by him only. All the earlier botanists in California noticed the exceeding paucity of mosses in that part of the continent, and some of the most skillful collectors have declared that mosses are almost absent from the flora; only about 50 species having been found within the state before the geological survey began its labors in 1860. Mr. Bolander alone has since collected nearly 200 species, and he believes that when the region shall have been as fully explored as are similar regions in the eastern States and Europe, that the Pacific States will be not less rich than they in species of this order. It is an interesting illustration of how much may be done by one man to correct the errors of too hasty generalization.

8. *A Botanical Semi-centennial.*—On the 20th of December last a supper was given at the Astor House to Dr. John Torrey by the Botanical Club of New York, it being just fifty years since he presented to the Lyceum of Natural History of New York his "*Catalogue of Plants growing spontaneously within thirty miles of New York City.*" Besides the members of the Club, there were present by invitation, Prof. Gray, Col. Olney, Prof. Brewer, Prof. Eaton, Dr. Pickering, Mr. Thos. P. James, Drs. Green and Porter of Easton, Pa.,

Mr. Parsons of Flushing, Mr. Buchanan of Astoria, and Mr. C. F. Austin of Closter, N. J. Letters were read from many prominent Botanists regretting their inability to come.

After the supper, Prof. Thurber called the meeting to order, and proposing the health of Dr. Torrey, gave a detailed account of his botanical labors. Dr. Torrey followed, first acknowledging his appreciation of the unexpected compliment of this supper in his honor, and then telling of the great difficulties he formerly encountered in the prosecution of Botanical Science. Prof. Gray, Prof. Eaton, Dr. Pickering, Prof. Brewer, Mr. Wm. Leggett and Mr. James Hogg responded to the other regular toasts of the evening, after which other gentlemen were called upon for speeches. Mr. James produced the manuscript journal of Frederick Pursh, made during his American travels. This journal has lately been discovered in the library of the Philadelphia Academy, and proves to be of such interest that its publication by the Academy is greatly to be desired. Prof. Gray remarked that he had rescued from oblivion also, the hand-drawn map which belonged to the journal. Among other points of interest, it may be noted that this journal describes the exact place where Pursh discovered the Scolopendrium.

The occasion was one of great interest to those who were present, and all who spoke testified to the great kindness with which Dr. Torrey had always aided their botanical efforts. May the spirit shown by this eminent man be cherished by all who shall follow in the path he has opened to American botanists.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Die trockene Distillation des Holzes und Verarbeitung der durch dieselbe erhaltenen Rohproducte auf feinere.* Ein Handbuch für Techniker, Chemiker, und Fabrikanten. Nach eigenen mehrjährigen Erfahrungen bearbeitet von Dr. EDUARD ASSMUSS. Berlin, 1867. 8vo, pp. 144.—Among the often wasted products that are capable of being turned to good account, none is more worthy of attention than wood smoke; especially in a country in which a heavy excise duty on spiritous liquors almost amounts to a prohibition of the manufacture of the acetates by means of the oxydation of alcohol. But if the charcoal-burner has desired information respecting the utilization of the condensible vapors, he has hitherto been obliged to put up with the meager and ill-digested accounts scattered in books of general chemical technology; for the work before us is the only one that has ever been published, specially devoted to the subject of which it treats. The author having been chemist and superintendent of a manufactory in Western Russia, of the products obtained by the destructive distillation of wood, should be well fitted for bringing together all that is known respecting the matter in hand. He aims to give a practical treatise and to detail only such methods as his own experience has proved good. He has very wisely avoided the too common absurdity of swelling the size of a mere technical work by useless preliminary chapters

on general chemistry, and, indeed, has confined himself to rather too narrow a range; for much might have been added which would have been altogether pertinent to the subject, and at the same time interesting and profitable to his readers. Greater completeness might have been attained by giving some account of methods and apparatus recommended by others, and by noticing products likely to prove useful in the arts. Thus in treating of the acetates possessing a commercial value, acetate of baryta would seem deserving of mention, since this salt may, in many cases, well replace the more costly acetate of lead, and it can be easily manufactured pure, even from very crude materials.

Re-distilled pyroligneous acid neutralized with witherite or with sulphid of barium and evaporated almost to dryness, with constant stirring, gives a dirty, granulated product which may be purified by washing it with pretty strong alcohol. The alcohol dissolves the gummy and tarry matter and leaves the acetate clean and of a pale brown color. When these brownish crystals are dissolved in water and treated with a little bone black, we obtain by filtration a colorless solution, which, on being dried down affords a nice white salt. So dense is acetate of baryta, that but small quantities of alcohol are required for systematic washings; and it is not unlikely that the alcohol might be replaced by wood spirit. Of course the alcohol may be recovered by distillation, and used again and again.

The manufacturer who would carry on his work intelligently should know how to determine the strength of his products by some way giving more certain indications than the mere hydrometer; and the entire omission of everything relating to this matter seems to us a defect in the book before us. A ready mode of testing pyroligneous acid is a desideratum, for the acid itself assumes a very deep color when neutralized, and the common acidimetric method depending on the change of litmus, is therefore precluded. Litmus is at best but an indefinite indicator of the neutralization of acetic acid. In trying transparent and light colored vinegars it is a good plan to add a few drops of a solution of neutral sulphate of copper and then proceed with the addition of a normal alkali. The least excess of alkali gives rise to a perceptible opacity in the liquid under examination. But pyroligneous acid becomes impervious to light so that even this artifice is unavailable. Perhaps the easiest course would be to ascertain with a delicate hydrometer, the increase of specific gravity caused by digesting the acid for some time with an excess of pure dry carbonate of lime, carbonate of baryta, or oxyd of zinc, tables having been constructed according to exact analyses, to show the percentages of acid corresponding to every increase of density. But no testing whatever should be entrusted to a mere practical man, until he has been specially instructed in the proper routine by some one well skilled in chemical manipulation. We were forcibly impressed with this thought by an instance that came under our notice a few years ago. A vinegar maker having sold a large quantity of his vinegar, warranted to

be of such strength that each fluid ounce should require for its neutralization fifty grains of carbonate of potash, was surprised to be told that it was found to fall far short of the guarantee. Not satisfied with the report of a professed chemist, he went to the storehouse of the buyer to prove that he himself was right, and took his carbonate of potash with him. Getting an average from many barrels, he measured out a fluid ounce. Then weighing fifty grains of the carbonate, he added it little by little, to the *cold* vinegar till the effervescent solution turned litmus paper blue, and had two grains left. The chemist repeated the experiment, heating the vinegar, of course, and showed him that by proper manipulation only twenty-nine grains of carbonate were required. Moreover, the carbonate was in suspiciously large crystals and turned out to be pure bicarbonate, twenty-nine grains of which would be equivalent to but twenty-five grains of the crystallized carbonate. This manufacturer is not the only one who has for years rested satisfied with cold testing by something supposed to be carbonate of potash, but really of unknown strength.

The author gives a pretty full description of the wrought iron retorts which he himself has used in charring wood; but in size and general arrangement they do not seem to us to be specially worthy of imitation. He also describes a judicious modification of Reichenbach's kiln, which he prefers for operating on the largest scale, because, as he says, the horizontal iron retorts soon burn out, and they allow but small quantities of wood to be distilled at once. The first cost of iron is great, especially in regions where wood is cheap, and those who have wood in abundance and wish to carry on a large business, would do well to try something like the kiln which he recommends. Rectangular and circular kilns of brick are most commonly used in this country for the simple charring of wood, but not when the acid is the chief object of the operation. Our makers of pyroligneous acid distil wood in iron retorts, and some even so far blindly copy what they have seen that they very absurdly have the cylinders six feet long, though the wood is brought in lengths of four feet and consequently a part has to be sawed in two to fill out the odd two feet. Others show more judgment in regard to size, and after trying various forms, consider it most advantageous to have cylinders cast in flanged quarters and bolted together. But retorts to be heated merely by an external fire cannot well be more than four feet in diameter, and are seldom made so large, so that it takes a great number to do much work. Some of our gunpowder manufacturers char their wood in an apparatus which in some measure combines the advantages of the kiln with those of the older form of retort. It consists of a cylinder whose sides are made of quarter-inch boiler plate and riveted to heads of cast iron. Through the lower part, about eight inches from the bottom, runs horizontally a very thick cast iron flue consisting of a fire-place one foot in diameter, and four feet long, bolted to the front head, and an extension six inches in diameter slipping loosely through a hole in the back head. There is a

hole in the front head corresponding to the diameter of the fire-place and closed by a door swung on hinges. To a hole in the upper part of the back head is attached a pipe six inches in diameter for carrying the vapors to the condenser. In the upper part of the front head is a hole about three feet square, for charging and discharging, which is closed by a flat cast iron door hung on hinges and held tight by a cross bar and wedges. The retort is so set in brick work, that when the flame from the wood burned in the fire-place has passed through the extension to the back end, it turns and wraps around the cylinder and passes into a chimney over the front end. The two pipes constituting the fire-place and its extension, have to be renewed every year or two, but the cylinder will last many years. As the charging door must necessarily be over the fire-place, these retorts are very inconvenient to discharge, and are not even easily charged. But the difficulty may be remedied by making the retort of greater diameter so as to give room for two internal flues instead of one. In an improved form the cylinder is made seven feet in diameter and thirteen feet long, with cast iron heads nearly two inches thick. In the central part of the front head is an opening extending from top to bottom and of such width as to allow a man to walk in and carry the wood for charging. In the lower part, on each side of this doorway, is a hole thirteen inches in diameter forming the entrance to the fire-place, which is a thick iron pipe a little over four feet long, or long enough to take in a common stick of wood. These fire-place apertures are furnished with hinged and latched cast iron doors. The charging aperture has a door not simply flat, but with returned edges like a shallow trunk lid. These returned edges add greatly to the strength and stiffness of the door and prevent it from warping in the casting or while in use. The door is hung on very stout hinges, and during the distillation it is kept closed by three cross-bars with set screws From each fire-place pipe, a cast iron flue eight inches in diameter extends just through the back head of the retort, and this slip-joint is kept tight by luting with clay or mortar. Only the lower part of the back head is exposed to the flame from the fire, the retort being set so that the flame turns, after leaving the internal flue, and passes forward under the lower half of the cylinder, then up and backward around the upper half to a chimney at one of the back corners of the brick work. To a hole twelve inches in diameter in the back head is attached a cast iron pipe which carries the volatile products of the distillation to a suitable condenser. Such a retort holds nearly three cords of wood and is quite as easily worked as a one-cord cylinder. The distillation occupies thirty-six hours and requires about one cord of wood for fuel, a quantity proportionally somewhat less than that consumed in heating the smaller retorts.

In the second division of the work, the author gives plain and concise directions for preparing dry acetate of lime, other acetates having commercial importance, acetic acid, and wood spirit. This book, like all others, throws no light on the interesting problem, how to purify pyroligneous acid without the roundabout and ex-

pensive process of converting it into acetic acid proper. The only practical answer that we have hitherto been able to obtain is but a partial one. If we distil the acid, treat the product with enough chlorid of lime to oxydize and precipitate the dissolved tar, about a pound of bleaching powder to five gallons of the distillate,—add sufficient muriatic acid to take up the lime, and then redistil,—we obtain an almost colorless vinegar which is pure enough for many uses in the arts, but still retains an unpleasant odor.

Lastly, the book treats of the very few soluble products which may be obtained from the tar, and of the sanitary aspects of the whole business.

Though the work contains little that is really new, and some of the methods given are susceptible of improvement, the manufacturer of progressive disposition will find it well worth reading, and he may get from it some hints which will tend to a bettering of his own plans. J. M. O.

2. *Reclamation by Eug. W. Hilgard.* (From a letter to the editors, dated University of Mississippi, Oxford, January 8, 1868.) —In Budge's Handbook of Physiology, edition 1862, page 940, where the distinction between "savors" and "flavors" is discussed, the entire credit of the discovery and explanation of these facts is given to Dr. Schinner, who in a "Dissertatio," 1856, and in "Deutsche Klinik," 1859, puts forth as new, a portion of what, so long ago as 1849, was demonstrated before a class of fellow students and upon no less a person than that of Prof. Ludwig, then at Zürich, by my brother, Dr. Theod. C. Hilgard (now of St. Louis) and myself; was subsequently made the subject of my brother's inaugural thesis, maintained before the faculty at Würzburg, in 1852; then formed the substance of a paper read before the American Association at the meeting of 1854, and published in that year's proceedings.

These dates suffice to establish priority even as to publication, on my brother's part, which priority would be greatly antecedent had not Prof. Ludwig failed to realize the importance of the experiment to which he himself was subjected, by attributing his failure to perceive the flavor of the garlic placed in his mouth to a benumbing of the gustatory nerves by the pressure of the unrelenting fingers which closed his nostrils spite of his spasmodic efforts to free them, to "nervous consensus," and the like. He therefore neglected the entire subject in his text-books published at the time; but the matter got wind, notwithstanding, so that when my brother spoke of it to Prof. Virchow at Wützburg, three years after, the professor observed that it was not entirely new, it having been heard of *from Zürich.* My brother's experiments on the subject (in most of which I participated, having started them in consequence of a suggestion made to me in 1848, by Prof. M. Temple of Philadelphia) were carried on, and made no secret of, successively at Heidelberg, Zürich and Vienna, which, together with the thesis at Würzburg, and publication as just stated, renders Dr. Schinner's originality a little questionable. Strangest of all, it

took thirteen years from the time these important distinctions were established and divulged by Americans to the highest authorities on the subject of Physiology, then recognized on the continent, to make an impression on European physiologists so far as to lead them to put it into a text-book. This is not, surely, the spirit likely to promote the progress of true knowledge, and has too frequently the effect as well as the appearance of a *suppressio veri.*

EUG. W. HILGARD.

University of Mississippi, Jan. 8, 1868.

3. *On the spectra of the meteors of November* 13–14, 1866; by JOHN BROWNING, Esq.—To view the shower I chose the observatory of Mr. H. Barnes, at Upper Holloway. The situation was good, the observatory being built on high ground and so placed that the radiant-point rose in the contrary direction to the lights of London.

I devoted my attention exclusively to attempting to obtain the spectra of as many meteors as possible.

After catching a few spectra in different directions, I at length decided on keeping the direct-vision prism pointed a little to the west of Leo Major, with the axis of the prism parallel to the horizon. The spectra which I saw were those of meteors which started from the radiant-point and passed through the belt of Orion. Of course the number of meteors which came into my field was comparatively limited; but the whole of them travelled in a direction parallel to the axis of the prism, a condition essential in the observation of the spectra.

From the rapid flight of meteors rendering the spectra very difficult to catch, I cannot pretend to speak with confidence of the appearance of the spectra shown by the prism; but I saw a great difference between the spectra; I believe that I saw spectra of the following kinds:—

A. Continuous spectra, or those in which the whole of the colors of the solar spectrum were visible excepting the violet rays.

B. Spectra in which the yellow greatly preponderated, but which in every other respect resembled those above described.

C. Spectra of almost purely homogeneous yellow light, but with a faint continuous spectrum—that is, a faint trace of red on one side and green on the opposite side of the yellow portion of the spectrum.

D. Spectra of purely homogeneous green light; of this kind I only saw two.

I observed through the prism spectra of several trains. The light, which was mostly blue, green, or steel-grey, generally appeared homogeneous; but this may have arisen from the light having been too faint to produce a visible spectrum. Stars below the second or third magnitude, although visible through the prism, fail from this cause to give spectra in which blue and red are perceptible.

It will probably be remarked that I have not spoken of having observed any lines in the spectra. All the nuclei seemed to give

continuous spectra which contained the whole of the colors of the spectrum; what I should term the tails, not the trains, of the nuclei presented the appearance I have described. In every instance I remarked that orange-yellow appeared to preponderate over the other colors in the continuous spectra. When a prism only is used, it seems to me impossible that any sharply-defined lines should be shown. Still, from differences in the colors of spectra, some information may, I think, be obtained. As is well known, chemists and mineralogists infer the presence of certain elements in the substance under analysis from the color communicated by the substance to the blowpipe flame. Thus, if the flame become yellow, they suspect the presence of sodium; red, strontium; green, barium or thallium; lavender, potassium. The prism will do more than this; it will show if the flame contains even a faint trace of any other color, while without the prism the faint color would be completely masked by the color which predominates. From the difficulty of catching meteors within such a narrow space, I fear it will be found impossible to use a prism provided with a slit formed by a pair of knife edges so as to define any lines. But I think it may be possible to use a prism in connection with a cylindrical lens. Such an arrangement would be capable of showing well-defined lines if the observed meteors contained any elements which would give bright lines in an ordinary spectroscope.—*Monthly Notices of the Royal Astronomical Society*, Jan. 11, 1867.

4. *National Academy of Sciences.*—At the recent January meeting of the Academy, held in Washington, the following papers were read.

On the practical character of the usual thermometer scales, and a common substitute for them; by A. Guyot.

The use and interpretation of single and double linear associative Algebras; by Benjamin Peirce.

On the existence of a great central zone of fracture of the surface of the globe; by A. Guyot.

A communication respecting the cosmical theory of the November meteors; by A. C. Twining.

The history of a week in the life of a young Salmon; by L. Agassiz.

On the Fauna of the Gulf of Mexico at great depths; by L. Agassiz.

Remarks on the Buffalo of North America; by L. Agassiz.

On the penetration of sound; by Joseph Henry.

Some points in the geological development of the continent between the Rio Grande and Arkansas river; by John L. LeConte.

On a reflector to be used as a signal in geodetic operations; by J. E. Hilgard.

Exploration of the Coast of Alaska; by Geo. Davidson.

Comparison of the iron meter belonging to the American Phil. Soc., and used as the standard measure of the Coast Survey, with the platinum standard meter of the "Conservatoire des Arts et Métiers" in Paris, in the summer of 1867; by F. A. P. Barnard.

On observations of the zodiacal light; by Benjamin Peirce.

Notes on a treatise on topographical drawing, by J. Enthoffer; by J. E. Hilgard.

Remarks upon the condition of the skeleton of Mastodon found at Cohoes, N. Y., and its relations to the geology of the surrounding country; by James Hall.

Solution of a particular form of Algebraic equation; by Benj. Peirce.

Note on the velocity of transmission of perceptions and volitions; by B. A. Gould.

5. *On the Miocene Flora of North Greenland;* by Professor Oswald Heer. (Translated by Robert H. Scott.)—Among Prof. Heer's important results is the following. The flora of Atanekerdluk proves, without a doubt, that North Greenland, in the Miocene Epoch, had a climate much warmer than its present one. The difference must be at least 30° F.

He says that the evidence from Greenland gives a final answer to those who objected to the conclusions as to the Miocene climate of Europe drawn by him on a former occasion. It is quite impossible that the trees found at Atanekerdluk could ever have flourished there if the temperature were not far higher than it is at present. This is clear first from many of the species, of which we find the nearest living representatives 10° or even 20° of latitude to the south of the locality in question. Some of the species are quite peculiar, and their relationship to other forms is as yet in doubt. Of these the most important are a *Daphnogene* (D. Kanii), the genus *M'Clintockia*, and a *Zamites*. The Daphnogene had large, thick leathery leaves, and was probably evergreen.

M'Clintockia, a new genus, comprises certain specimens belonging, perhaps, to the family of the Proteaceæ. The *Zamites* is also new. Inasmuch as we know no existing analogues for these plants, we cannot draw accurate conclusions as to the climatal conditions in which they flourished. It is, however, quite certain that they never could have borne a low temperature.

If, now, we look at those species which we may consider as possessing living representatives, we shall find that, on an average, the highest limit attainable by them, even under artificial culture, lies at least 12° to the southward. This, however, does not give a fair view of the circumstances of the case. The trees at Atanekerdluk were not all at the extreme northern limit of their growth. This may have been the case with some of the species; others, however, extended much farther north; for in the Miocene flora of Spitzbergen, lat. 78° N., we find the beach, plane, hazelnut, and some other species identical with those from Greenland. For the opportunity of examining these specimens, I am indebted to Professor Nordenskiöld. At the present time the firs and poplars reach to a latitude 15° above the artificial limit of the plane, and 10° above that of the beach. Accordingly we may conclude that the firs and poplars which we meet at Atanekerdluk and at Bell Sound, Spitzbergen, must have reached up to the north pole, in so

far forth as there was land there in the Tertiary period. The hills of fossilized wood found by M'Clure and his companions in Banks Land (lat. 74° 27′ N.) are therefore discoveries which should not astonish us; they only confirm the evidence as to the original vegetation of the polar regions which we have derived from other sources.

He then selects *Sequoia Langsdorffii*, the most abundant of the trees at Atanekerdluk, and proceeds to investigate the conclusions as to climate deducible from the fact of its existence in Greenland. *Sequoia sempervirens* Lamb. (Red-wood) is its present representative, and resembles it so closely that we may consider *S. sempervirens* to be the direct descendant of *S. Langsdorffii*. This tree is cultivated in most of the botanical gardens of Europe, and its extreme northern limit may be placed at lat. 53° N. For its existence it requires a summer temperature of 60° F. Its fruit requires a temperature of 65° F. for ripening. The winter temperature must not fall below 31° F., and that of the whole year must be at least 50° F. Accordingly we may consider the isothermal of 50° as its northern limit. This we may then take as the northern temperature of the *Sequoia Langsdorffii*, and 50° F. as the absolute minimum of temperature under which the vegetation of Atanekerdluk could have existed there.

The present annual temperature of the locality is about 20° F. Dove gives the normal temperature of the latitude (70° N.) at 16° F.

Professor Heer concludes his paper as follows:—

I think these facts are convincing and the more so as they are not insulated, but confirmed by the evidence derivable from the Miocene Flora of Iceland, Spitzbergen, and Northern Canada. These conclusions, too, are only links in the grand chain of evidence obtained from the examination of the Miocene Flora of the whole of Europe. They prove to us that we could not by any re-arrangement of the relative positions of land and water produce for the northern hemisphere a climate which would explain the phenomena in a satisfactory manner. We must only admit that we are face to face with a problem, whose solution in all probability must be attempted and, we doubt not, completed by the astronomer.

OBITUARY.

Christian Gottlieb Ferdinand Engel, born at Magdeburg in Prussia on the 8th of March, 1805, died on the 12th of July, 1866, at Tremont near New York. A man of singular beauty and simplicity of character, an artist and a scientist, he well deserves a few words in remembrance of his honorable and useful life. Mr. Engel received his first instruction at a public school in his native city. The stern will of his father compelled him to learn the trade of a carpenter, but he devoted himself without his father's knowledge to the study of mathematical drawing, and at the age of sixteen sent to the Academy of Arts, in Berlin, a work which received the highest prize. Surprised and touched, his father no longer opposed the youth's wishes; he entered the Royal School of Arts in Magdeburg, and at the conclusion of his studies, bore off the most

honorable testimonials. He settled at first in Magdeburg as a teacher of drawing, removed thence to Hamburg, where he taught descriptive geometry, and finally went to Berlin, devoting himself with extraordinary industry and success to the work of instruction. His lectures upon geometry and optics soon became popular and were attended by enthusiastic students from all classes in life. In 1835 he prepared the constructions and drawings for Wolff's descriptive geometry, and some years afterward published, in connection with Prof. Schellbach, two volumes of descriptive optics, to which Prof. Schellbach contributed the text and Mr. Engel the drawings. The work is one of extraordinary beauty and value, but is, we believe, but little known in this country. Mr. Engel's optical studies soon led him to the wave theory of light; he first drew and then executed in wood a model of the wave surface in biaxial crystals. Of these models Prof. Plücker in Bonn received the first; the second was sent to the great Exhibition in London, where Mr. Engel received a prize medal. He afterward published axonometrical projections of the most important geometrical surfaces and sheets of copies for descriptive geometry.

In 1848 the Revolution broke out in Germany, and Engel embraced the liberal side with all the enthusiam of his nature. How sadly the hopes and aspirations of the German liberals were defeated, we all know. Disappointed, disheartened, and persecuted in the political reaction which followed, Mr. Engel sought a more congenial home in this country. For nearly eighteen months after his arrival misfortune seemed to pursue him; his scanty savings rapidly wasted, no means of support offered themselves, and he had formed the resolution to go to England when he made the acquaintance of a few men of scientific tastes and pursuits who appreciated the beauty of his work, and who could not but admire his simple, modest, unobtrusive character. He now found occupation in making optical models of various kinds in wood and plaster. Of these perhaps the most beautiful are the model of the wave-surfaces in biaxial crystals and a wooden model exhibiting the passage of a plane through all the varieties of elliptic to circular vibrations and back again to a plane vibration at right angles to the first. The attention of the late Prof. Bache having been directed to Mr. Engel's drawings and models, the latter received an invitation to enter the service of the Coast Survey. In 1856 he undertook the drawing of the instruments and apparatus of the survey for the great work which Prof. Bache had projected, and even engraved the outlines on copper with his own hand. During the last years of his life he was occupied privately with the projection of curves of equal illumination, hoping in this manner to place topographical drawing upon a scientific basis.*

Science in our country has met with a real loss by the death of Mr. Engel. He possessed in a higher degree than any person whom we have ever known, a true geometrical sense. He solved

* Mr. Engel left a large collection of optical and geometrical models which are now on sale for the benefit of his widow, and which may be seen at 9 Lafayette Place, New York.

difficult problems with the greatest readiness by construction alone, and his models are marvels of beauty and precision of workmanship. His skill with the drawing pen was unequalled, and it would be difficult to find anything more beautiful in their kind than his drawings of caustic curves, in the work above referred to, and some unpublished optical sketches which we have had the privilege of seeing. W. G.

Sir DAVID BREWSTER, K. H.—Sir David Brewster died at the age of 86 on the 10th of February last.

V. MISCELLANEOUS BIBLIOGRAPHY.

1. *The American Ephemeris and Nautical Almanac for the year* 1869. Published by authority of the Secretary of the Navy. Bureau of Navigation, Washington, 1867. Large 8vo, pp. 516, with Appendix, pp. 40.—This is the 15th annual issue of the American Ephemeris and Nautical Almanac, the first volume having been that for the year 1855. The first 248 pages forms the Nautical Almanac, and is intended for the use of Navigators. It is for that object computed in Greenwich time. The remaining portion is Astronomical, and is constructed for Washington time.

It is the province of such a work to predict astronomical phenomena, especially to give those predictions that supply the practical necessities of navigators, exploring expeditions, boundary commissions, coast and lake surveying parties, and astronomical observers generally.

The necessity of such publications, and the propriety of their issue by government, is evident from the simple fact, that almost all civilized nations issue them annually, even though their commerce, their geographical explorations and surveys, and their astronomical establishments, in most cases, bear no comparison in extent with those of our country.

The oldest series is the *Connaissance des Temps*, published by the *Bureau des Longitudes* of the French Government. This was first issued in 1679, so that the volume for 1869 is the 191st of the series. The *Nautical Almanac and Astronomical Ephemeris* published by the Lords Commissioners of the Admiralty of England, was begun in 1767, the volume for 1871 making the 105th of the series. Ninety-four volumes of the *Berliner Astronomisches Jahrbuch*, more than ninety volumes of the *Effemeridi Astronomichi*, of Milan, more than fifty of the Russian Nautical Almanac, as well as the several series from Vienna, Coimbra, Lisbon, Madrid, Rio Janeiro, and other places, show either that the necessity for such a publication has been felt among all the nations thus represented, or else that it was considered a discredit to a country to be dependent for a nautical almanac upon a foreign government.

Unquestionably at the present day the best known and most important of these Ephemerides are the British *Nautical Almanac*, the *Berliner Jahrbuch*, the *Connaissance des Temps*, and the *American Ephemeris*. Those of Milan and Coimbra have an excellent reputation, but can hardly be placed in the first rank. In the

form of the tables the excellent arrangement of the British Almanac, devised in 1830 by a committee of the Royal Astronomical Society, has been most wisely followed by the American Ephemeris. Changes from a system so well known in our marine, ought not to be made without great reason. Yet in the details there are considerable differences. The tables employed are generally different. For the Sun the Nautical Almanac uses LeVerrier's, the American Ephemeris, Hansen and Olufsen's. For the Moon they use severally Hansen's and Peirce's. For Mercury, Venus and Mars, the former employ LeVerrier's tables, for Jupiter, Saturn and Uranus, those of Bouvard, and for Neptune those of Kowalski. In the American Ephemeris, Winlock's tables of Mercury, Lindenau's tables of Venus and Mars (with corrections by Airy, Breen and LeVerrier), Bouvard's of Jupiter, Saturn and Uranus, (with corrections) are used. For Neptune, Peirce's theory and Walker's orbit are adopted. In the Appendix, is an Ephemeris upon the later orbit of Newcomb. To the usual Ephemeris of the planets, is added a table of the rectangular coördinates, of use in computing perturbations.

In the places of the fixed stars there are still greater differences. The apparent places for the upper transit for every ten days are given in the American Ephemeris for 198 stars, in the Nautical Almanac for 147, in the Connaissance des Temps for 116, and in the Berliner Jahrbuch for 72 stars. Hardly any portion of the Almanac is more useful to the practical astronomer, whether on an exploring expedition, on a surveying party, or in a fixed observatory. In another point the American Ephemeris surpasses all the others, namely, in the materials for computing occultations. There is in this a special appropriateness, as there are so many parties in all portions of our wide country, who need to observe these occultations to determine the longitudes of their stations.

The volume for 1869 has been prepared under the direction of Prof. J. H. C. Coffin, those of the years immediately preceding by Prof. J. Winlock, now of the Cambridge Observatory. Only an astronomer can direct such a work, and secure that accuracy without which it is worse than useless, and we trust that the post of superintendent will never be regarded as one to be conferred in routine upon officers who are not astronomers.

We learn with regret that the Committee on Appropriations of the House of Representatives has felt it necessary to reduce the small amount estimated for the Ephemeris, by nearly one-half. We cannot believe that a permanent reduction of the size or character of the Almanac is intended. Too many material interests are concerned, and the credit of the country is too deeply involved. But if the diminution of the appropriation is not to be permanent, it is very unfortunate that it is to be made at all. The volumes should be issued two or three years in advance, and to accomplish this the appropriations must be regular. Under its past and present able direction the almanac is such, as to compare favorably with any similar publication in the world. But irregular or inadequate support will surely prevent its maintaining a character creditable

to a country with such large maritime interests, such a territory to survey, and we may add, one that is so honorably known for its astronomical science.

2. *Sound: A course of eight lectures, &c.;* by JOHN TYNDALL, LL.D., F.R.S., &c. &c. London. Longman, Greens & Co. 1867. —This work on sound by Dr. Tyndall, is a popular exposition of the leading phenomena of sound and especially of what may be termed *Opt-acoustics.* It is a stenographic report of eight lectures delivered at the Royal Institution. Such books have nearly the life and freshness of the spoken lectures, while they have this advantage over the latter, that the experiments never miscarry, and that all who are addressed have an equal opportunity of seeing and hearing.

The general public has heretofore been compelled to rely for its knowledge of science too largely upon the productions of third, fourth or *n*th rate men, who often furnish rhetoric (falsely so-called) for truth and fancy for fact. This necessity is fast disappearing. By this volume and by his previous one upon heat, Dr. Tyndall has set an example of condescension to the wants of the age, worthy of imitation by others of our masters in science. Dr. Tyndall has indeed rare gifts for a popular teacher. Philosopher, orator and poet (for he has written much poetry, though no verse), his lectures are at once exact, clear and beautiful. As a specimen of his power of lucid exposition, we cite his account of LaPlace's mode of reconciling the observed and calculated velocities of sound in air (pp. 30–32). As one among many instances of a command of felicitous expression scarcely ever seen outside of poetry, we refer to his description of Wheatstone's telephonic concert, (p. 81).

In his lectures on heat, the author appeared more prominently as a *discoverer.* In the present work, original labors are not wanting, but he is mainly an expositor of the researches of others, and most conspicuously of Helmholz.

The true source of "timbre" in musical sounds, the composition of vowel sounds and the origin of resultant tones, as given in the remarkable work* of the latter philosopher, are here satisfactorily presented. We miss, however, with some surprise, even in a popular account of Helmholz's discoveries, what is perhaps the most significant and the least anticipated of them all; namely, that the timbre of a musical sound is independent of the difference of phase under which its constituents are united. Before Helmholz, men had attributed timbre to the union of subordinate sounds with the fundamental tone. (See Daguin, tome 1, pp. 469, 485.) But it was universally held and taught, we believe, that the origin of timbre probably lay in the peculiar orbit of the vibrating particles of the soniferous medium, because the periodic time of these particles being appropriate to pitch and to amplitude of their excursions (orbits) to intensity, there was really nothing left for 'timbre' but the configuration of the orbit. Helmholz's discovery of the independence of timbre and phase, shows that difference of timbre

* Die Lehre von den Ton empfindungen, 1865.

is accompanied by difference in the orbital motion of the particles of the sound-conductor, yet identity of timbre is compatible with marked difference in the distribution of density in the sound-wave and so in the extension of the particles. How beautifully is the physical law explained by even the partial knowledge we have of the nature and structure of the labyrinth of the human ear! Helmholz finds in Corti's organ and Schultze's bristles, an instrument adapted to analyze a complex sound, without reference to the *phase* of its components. Dr. Tyndall strangely notices this structure of the ear, without adverting to the remarkable physical law which it explains.

In the midst of so much that is true and beautiful in the book, it is almost invidious to notice minute defects, yet we must say that the word '*obertoene*' seems not to be happily rendered by *overtones*. The latter word suggests *dominant* rather than *higher* tones.

It is to be remarked too that the snow-fall, mentioned on page 19, appears to have had an effect upon *timbre*, not noticed by the Dr. and indeed previously entirely unsuspected. It changed good French into bad French. But perhaps the guides fell into a crevasse, in which case the exclamation '*nous sommes finis*' was quite natural. S.

3. *Guyot's Geographical Text Books;* 1, Primary; 2, Intermediate; 3, Common School. New York, C. Scribner & Co.—These three volumes are likely to attract the attention of all who see them because of their beautiful engravings and maps, and because of their clear and satisfactory typography; but these are minor merits. The true value of the series consists in the fact that it is prepared by one who is a master of the science which he treats and who is also a skillful and experienced teacher, well acquainted with the requirements of American schools. Such text-books are so rare in the educational literature of this country, and the number of "manufactured" text-books, prepared by incompetent persons is so great, especially in elementary science, that all who are interested in the promotion of thorough education and the diffusion of true science should welcome the appearance of new manuals which can honestly and heartily be commended. The highest praise in our opinion is due to the Geographical series of Professor Guyot. If the largest volume of the three could be made the basis for a thorough examination of candidates for admission to the college and the scientific school, we are confident that the higher education of the young men would be much easier than it is at present, when their geographical training is admitted to be disproportionately defective.

The Guyot books are the only geographies which we know of in the language recognizing throughout, the fundamental, natural characteristics of every region on the globe. The scholar is made familiar at the outset of his studies with the aspects of Nature, and he is soon led to discover that the physical peculiarities of the different countries underlie, and to some extent control national and political characteristics. This familiarity is carried at every step into more and more detail. We shall not be surprised if teachers who are only familiar with such geographies as chiefly tax the

memory by requiring the acquisition of long lists of names, at first pronounce the Guyot series strange and hard; but we are persuaded that if they will carry a single class through either book, they will find that the reasoning faculties of the pupil have been trained as well as the memory; and that the powers of judging, comparing and of image-forming has been very greatly enlarged. Since Geography is the one study involving an acquaintance with the world of nature, which has been gradually introduced into the common schools, it ought to be taught on right principles, and now for the first time among us this is possible.

The maps which illustrate the Guyot books are admirably prepared, so as to exhibit at once, natural features and political divisions. They are free from unnecessary details and present important general truths in a bold, impressive manner. Some of these maps are the best cheap maps which can readily be obtained for any purpose, those of our own country having original merits of especial value. We have repeatedly commended them to those who were in search of a trustworthy atlas at a moderate price.

In the literary preparation of these books, Prof. Guyot has been assisted by Mrs. Mary H. Smith, of the Oswego Training School, a very competent teacher, and thus it is thought that his books have been made still more acceptable to American instructors.

The wall-maps, which are issued by the same publishers, and are prepared by Mr. Guyot and his associate, Mr. Landoz, are an excellent accompaniment of the text-books, and they are of great independent value. They may be had in any one of three dimensions according to the room for which they are designed. The larger sets are admirably adapted to college lecture rooms and academies, and the smaller to ordinary schools.

4. *A Journey in Brazil;* by Professor and Mrs. Louis Agassiz. 540 pp. 8vo, with several plates. Boston, 1868. (Ticknor & Fields.) —This work on the Brazilian expedition of Professor Agassiz, whose departure was announced in the volumes of this Journal for 1865, has a double interest from its joint authorship. The Journal of Mrs. Agassiz is full of interesting incident and description, written in a graceful and attractive style. Prof. Agassiz has added to the chapters the results of many of his important scientific observations, made by sea and land, on his favorite topics, fishes and other life of the sea and rivers, distribution of species, glaciers, and general geology. A book notice does not afford space for presenting or discussing the views brought forward. The work is published in the best style, and is illustrated by plates giving excellent views of the scenery about Rio Janerio, Para, and the plains of the Amazon; though wood-cuts, several of them might be taken for steel-plate engravings.

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